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PART I.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, itwas thought adviſeable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

CONTENTS

OF

VOL. LXXII. PART I.

I. *RELAZIONE di una nuova Pioggia, ſcritta dal Conte* de Gioeni *abitante della 3ª Reggione dell' Etna; communicated by Sir* William Hamilton, *K. B. F. R. S.* page 1

II. *Nova experimenta Chemica quæ ad penitiorem Acidi e Pinguedine eruti cognitionem valere videntur. Scribebat* D. Laurentius Crellius, Gulielmo Huntero, *M. D.* p. 8

III. *Obſervations on the Bills of Mortality at* York. *By* William White, *M. D. F. A. S.; communicated by* Nathaniel Pigott, *Eſq. F. R. S.* p. 35

IV. *Account of a monſtrous Birth. In a Letter from* John Torleſe, *Eſq. Chief of* Anjingo, *to the Hon.* William Hornbey, *Governor of* Bombay; *communicated by Dr.* Lind, *F. R. S.* p. 44

V. *Experiments with* Chineſe Hemp Seed, *In a Letter from* Keane Fitzgerald, *Eſq. to Sir.* Joſeph Banks, *Bart. F. R. S.* p. 44

VI. *An Account of ſome Scoria from Iron Works, which reſemble the vitrified Filaments deſcribed by Sir* William Hamilton. *In a Letter from* Samuel More, *Eſq. to Sir* Joſeph Banks, *Bart. P. R. S.* p. 50

VII.

VII. *An Extract of the Regiſter of the Pariſh of* Holy Croſs, Salop, *being a Third Decade of Years from* Michaelmas 1770 *to* Michaelmas 1780, *carefully digeſted in the following table. By the Rev. Mr.* William Gorſuch, *Vicar*; *communicated by Dr.* Price, *F. R. S.* p. 53

VIII. *An Experiment propoſed for determining, by the Aberration of the fixed Stars, whether the Rays of Light, in pervading different Media, change their Velocity according to the Law which reſults from Sir* Iſaac Newton's *Ideas concerning the Cauſe of Refraction*; *and for aſcertaining their Velocity in every Medium whoſe refractive Denſity is known. By* Patrick Wilſon, *A. M. Aſſiſtant to* Alexander Wilſon, *M. D. Profeſſor of Practical Aſtronomy in the Univerſity of* Glaſgow; *communicated by the Rev.* Nevil Maſkelyne, *D. D. F. R. S. Aſtronomer Royal.* p. 58

IX. *Quantity of Rain which fell at* Barrowby *near* Leeds. *By* George Lloyd, *Eſq. F. R. S.* p. 71

X. *Account of an improved Thermometer. By Mr.* James Six; *communicated by the Rev. Mr.* Wollaſton, *F. R. S.* p. 72

XI. *On the Parallax of the Fixed Stars. By Mr.* Herſchel, *F. R. S.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.* p. 82

XII. *Catalogue of Double Stars. By Mr.* Herſchel, *F. R. S. communicated by Dr.* Watſon, *Jun.* p. 112

XIII. *Deſcription of a Lamp-Micrometer, and the Method of uſing it. By Mr.* William Herſchel, *F. R. S.* p. 163

XIV. *A Paper to obviate ſome Doubts concerning the great Magnifying Powers uſed. By Mr.* Herſchel, *F. R. S.* p. 173

XV. *Continuation of the Experiments and Obſervations on the Specific Gravities and Attractive Powers of various Saline Subſtances. By* Richard Kirwan, *Eſq. F. R. S.* p. 179

XVI. *Del modo di render ſenſibiliſſima la più debole Elettricità ſia Naturale, ſia Artificiale. By Mr.* Alexander Volta, *Profeſſor of Experimental Philoſophy in* Como, *&c. &c.; communicated by the Right Hon.* George *Earl* Cowper, *F. R. S.* p. 237

XVII. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1780. *By* Thomas Barker, *Eſquire.* p. 281

XVIII. *Meteorological Journal kept at the Houſe of the Royal Society, by Order of the Preſident and Council.* p. 285

APPENDIX.

I. *Account of a new Kind of Rain. Written by the Count* di Gioeni, *an Inhabitant of the* 3*d Region of Mount* Etna; *communicated by Sir* William Hamilton, *K. B. F. R. S.* p. i

II. *Of the Method of rendering very ſenſible the weakeſt Natural or Artificial Electricity. By Mr.* Alexander Volta, *Profeſſor of Experimental Philoſophy in* Como, *&c. &c.; communicated by the Right Hon.* George *Earl* Cowper, *F. R. S.* p. vii

PHILOSOPHICAL TRANSACTIONS.

I. *Relazione di una nuova Pioggia, ſcritta dal Conte* de Gioeni *abitante della 3ª Reggione dell' Etna; communicated by Sir* William Hamilton, *K. B. F. R. S.*

Read November 8, 1781 *.

Volat per Mare magnum cinis decoctus, et terrenis nubibus excitatis, tranſmarinas quoque provincias pulvereis guttis implevit. CASSIOD. lib. IV. var. epiſt. 50.

LA mattina de 24 corrente ſi e qui preſentato uno de fenomeni piu' ſingolari; tutti li luoghi eſpoſti all' aria ſi trovarono bagnati da un' acqua colorita cretacea biggia, la quale

* For a tranſlation of this paper ſee the Appendix.

evaporando indi, o infiltrandosi nella terra, lasciò per ogni dove la materia che contenea, all' altezza di due, o tre linee; tutti i ferri, che ne furono tocchi, divennero rugginosi.

Il publico portato al maraviglioso, imaginò varie cagioni di tale pioggia, motivi di timore per gli veggetabili, e gli animali.

Le popolazioni, che si valgono delle acque piovane, si astennero di farne allora uso. Vi fu' chi sospettasse contenersi in essa de' principi vitriolici: e delle persone predissero un qualche male epidemico sopraveniente.

Chi avea osservato l'esplosioni, che l'Etna, da venti e piu' giorni, facea vedere dall' alto del suo cratere, inclinava a credere originato quel fenomeno da una di esse.

Si estese la pioggia dal nord ¼ nord est al sud ¼ sud-ovest sopra le campagne di noto, fin dove contansi in linea recta, settanta miglia, dal vertice dell' Etna.

Non e nuovo, che li volcani per la forza espansiva, che violenta in essi si genera, abbiano cacciato delle Sabbie * portate da venti di lontane reggione, e delle pietre †.

* L'autorita di Cassiodoro. premessa a questa relazione e avvalorata da Seneca nel suo 2° lib. delle Quest. Nat.

Ætna aliquando multo igne abundavit, ingentem vim arenæ urentis effudit, involutus est dies pulvere, populosque subita nox terruit.

Ma senza riandare alle moltissime memorie di questo volcano, e del Vesuvio, come a noi piu' vicini, abbiamo da 20 anni in qua veduto molte delle pioggie in Sicilia, originate dell' Etna, e l'ultima precedette la irruzione dell' anno scorso, era quella composta di piccioli frammenti di pomici bituminose, o fumie.

† La pietra, descritta da PLINIO caduta nella Tracia, la pioggia di pietra, che avvenne sul monte Albano, dopo la rovina di Alba, della quale ci fa menzione TITO LIVIO, e molte altre simili, rimarcate dalli antichi, come pioggie prodigiose, sono state riconosciute per volcaniche; in quanto all' Etna abbiamo a giorni nostri veduto formare de' monti nuovi, per il cumulo delle pietre, o per meglio dire delle lave; e degli antichi, oltre STRABONE, ed altri molti scrittori, il lirico PINDARO ci trasmette, che, *aliquando non tantum rivos igneos ejecit, sed saxa ignita.* PIND. ap Brit. lib. V, c. 14. p. 2.

Ma

Ma il colore della materia, e la sua sottigliezza, diedero motivi di dubitare, onde ne fosse originata, accrescendosene l'incertezza dalla rimarchevole circostanza dell' acqua, che portolla incorporata * e però si sospettava di altro principio.

Era dunque uopo per ogni ragione di assicurarsi della natura di tale materia, onde restar persuasi della sua origine, e degli effetti, che avrebbe potuto cagionare: non potea questo farsi senza il soccorso dell' analisi chimica: per far dunque ciò con siccurezza, procurai raccogliere quella pioggia in luogo, ove potessi credere, che non esistessero altre sostanze eterogenee: Scelsi perciò la pianta chiamata *Brassica Capitata*, la quale avendo le foglie larghe, e ravvolte, trovai, che conteneà a sufficienza dell' acqua colorita; e riversate molte di esse in un vaso, lasciai indi, che spontaneamente ne risedesse la terra al fondo, in cui dopo qualche tempo depose la parte limosa, restando l'acqua trasparente.

Separata questa in altro vaso, la tentai con de' liquori alcalini vegetabili, ed acidi minerali: ma non osservai decomposizione con alcuno de' due mestrui; passai ad evaporarle per riunire quelle materie, che potevano forse essere in soluzione, e toccatala di bel nuovo con gli anzidetti liquori, fece vedersi leggiera effervescenza con gli acidi; provata con lo sciroppo di viole, divenne questo verde smorto, così che mi persuasi, che contenesse un sale calcareo +. Con la decozione di galla non produsse precipitazione.

* Ne' molti scrittori del nostro Etna, non trovasi Pioggia di Sabbia, o di altra produzione mescolata con acqua.

+ Provata ancora con dissoluzione di piombo nell' acido vegetale, perdette il suo color naturale, e la sua trasparenza, divenendo lattiginosa. Io mi farei a credere, che sii quello un' effetto delle particelle alcaline, e così spiegare la efflorescenza, che mosse sopra li ferri esposti all' aria.

Diſſeccata poſcia all' ombra la materia, ſi fè vedere una terra ſottiliſſima di color cretaceo, ma inerte per eſſere ſtata diluta della pioggia.

Penſai calcinarla ad un fuoco leggiero, e vi preſe il color di mattone; poſta indi in un crogiuolo, una porzione di queſta la paſſai a fuoco piu' violente, e perdette quaſi il colore acquiſtato; un altra parte di queſta provata a piu' gagliarda, e lunga calcinazione (onde poteſſi ſperarne la vitrificazione) reſto cio non pertanto, frolle, e diviſiſſima, ritornando al ſuo priſtino colore biggio.

Oſſervato con accuratezza il fumo delle tré calcinazioni, non diede alcuno colore, ne odore, per ſoſpettarſi de meſcolanza arſenicale o ſulfurea.

Avuta dunque quella materia in tre porzioni, calcinata a tre differenti gradi di fuoco, le preſentai una buona magnete, ma non aggi queſta nella prima, e nella ſeconda; nella terza pero' una leggiera attrazione, in aghi viſibili, reiterata piu' fiate, mi fe ſtabilire, che ſia in queſta terra un principio marziale nella forma metallica, e non in ſoſtanza vitriolica *.

Della natura dunque delle materie riconoſciute, ſi rilieva la loro origine volcanica, imperoche il ferro piu' che e' eſpoſto a violenta calcinazione, perdendo il principio flogiſtico, piu' ſi rende diviſo, e non puo queſto ſuccedere naturalmente, che nel gran focolare di un volcano. Il ſale calcareo eſſendo un ſal marino, combinato con ſoſtanze calcaree per via di fuoco violente † non puo' altrimenti eſſere compoſto, che nel volcano ‡.

Percio,

* Imperochè non avrebbe altrimenti l'acqua prodotto effervescenza con gli acidi, ma l'avrebbe moſtrato con gli alkali, e nella triplicata calcinazione, ſi farà piu' toſto accreſciuto, che diminuito il colore roſſo.

† La combuſtione delle pietre da Calce puo' produrre, e' vero, la combinazione,

Percio, che appartiene alli temuti effetti, sopra gli animali, e gli veggetabili, è noto a chiunque, l'uso vantaggioso che ritira la medicina dell' uno e l'altro, in quella stessa forma che furono preparati nel gran laboratorio della natura.

Li veggetabili, che sono nell' attuale horificazione, non mostrano la menoma macerazione, come altre volte è avvenuto con le pioggie di Sabbia *.

Come poi quella produzione volcanica si sia mescolata all' acqua, puo cio concepirsi in varie guise.

L'Etna è ordinariamente attornato nella sua media reggione di nuvole, le quali non sempre oltrepassano la sua sommita, che si alza a 2900 passi † sopra il livello del mare, cacciatane fuori quella materia, trovando sottoposte le nuvole, potè avvenire, che si fosse mescolata alle stesse, e sciolta poi in pioggia nella maniera ordinaria: puo altrimente conghietturarsi, che quel denso fumo, che contenea la materia volcanica per la forza de'

zione, onde risulto il sale calcareo, ma scorgesi chiaramente, che non potea quella quantita altronde provenire che dal volcano.

‡ Molte, e replicate esperienze sopra li prodotti dell' Etna mi hanno persuaso, che il sal marino fii uno de principali e piu' abondanti mestrui, che eccitano le effervescenze del nostro volcano, o che ne fii la base (come un' amico di molta cognizione mi ha fatto nuovamente riflettere) trovo del sale calcareo nelle vecchie lave, del sale comune, lo trovo sublimato in ammoniaca nelle fenditure, e ne' spiragli delle nuove eruzioni; ma qui non è luogo á cio' che richiede un maggior volume, forse appresso potro' meglio dirne in altra occasione.

* Mi trovo aver replicatamente osservato, che le pioggie di Sabbia del nostro monte per lo piu' composte di materie calcinate, e di piccioli cristalli di Schorl, portano un cimento di particelle arsenicali, e sulfuree, e qualche volta saline, che unisce lo Schorl alle altre materie; cosiche se ne ingrossano li granelli; qualche volta ancora ci è arrivata la pioggia calda a terra.

† La misura, che ho tentato della perpendicolare del monte, mi è due volte riuscita all' altezza descritta, non pero la do per certa, sapendo che l'altimetria ha vopo di esatti istromenti, e di reiterate osservazioni, che dovro comprovare ancora con il barometro a miglior commodo.

venti, fosse trasportato nell' atmosfera, con la sua rarefazione, sopra quel tratto di paese *, e quindi raffreddandosi si sia condenzato tanto, che superando il peso dell' aria sottoposta, si abbia sciolto nella pioggia colorita.

Io rimetto per altro a fisici, a quali appartiene la cognizione degli aggenti della natura, lo esame, e la spiega di tale fenomeno, limitandomi alle osservazioni, ed alle esperienze di naturalista iniziato nella chimica, affine di concorrere con esse di qualunque merito siano, alla teoria de' volcani, e del globo †.

P. S. A 4 Maggio, Venerdi alle ore 21½ di Italia si e fatta sentire una scossa di terra assai leggiera, nelle abitazioni che sono attorno all' Etna, la quale piu' si rese sensibile in qualche lontananza del monte, la sua azione fu dal nord al sud. Avea il volcano continuato le fiamme, e le esplosioni, e la notte precedente, una colonna di fumo, composta di globi quasi articolati, l'uno sopra l'altro, si era alzata sopra il cratere all' altezza dupplicata della montagna per quanto facea arbitrare la distanza di

* Accio la addotta ipotesi non sembri esagerata per la quantita di fumo, che devesi supporre, io rapporto cio che fu osservato da CICERONE, cratere flamma erumpit, fumo mixta tam copioso, ut dum Boreas spirat Melitam usque per aerà illum sublimem propellat ad IX. millia passuum spatium. CIC. de Nat. Deor. lib. II.

† La Physique (dice il Sr. WALERIO nella sua mineral. t. 2. Hidrol. 2. f.) est plus universelle dans ses vûes, et plus philosophique dans son examen, le physicien envisage, raisonne, explique, le naturaliste regarde, ramasse, et range; celui-ci vous dira il existe tel corps dans la nature, il est fait, soit au dedans, soit au dehors de telle ou telle maniere, il est de tel ou tel regne, classe, ordre, espéce, variété; celui la pretendra vous expliquer les causes de son existence, de ses formes, et de ses propriétés.

Appresso al Sr. WALERIO l'illustre Sr. LINNÉ nell' amæn. transalp. anno 1740, ff. 2. cosi scrive; Physica est scientia de qualitatibus elementorum. Historia naturalis autem circa cognitionem corporum naturalium versatur: il vero naturalista dev' essere istruito della fisica, e della chimica ancora, ma non conosciamo ancor noi qui la divisione delle due scienze.

22 miglia dal vertice per linea retta, in cui è questa città; durò quella tutto la notte perpendicolare, sicchè si avea staccato uno de' globi, ed allungato all' ovest della sua cima; tratto tratto tutto l'interno della colonna, e della lingua prolungata di fumo venivano internamente illuminate da fuoco elettrico, che trasparia rosso cupo, estinguendosi gradatamente dal basso all' alto, in due secondi.

Ha continuato il fuoco sul cratere sin' oggi 8 Maggio, rigettando delle moli infuocate, le quali, vagamente rotolando giù per il cono, hanno illuminato quella reggione, e si è versata della lava dal cratere in qualche quantità verso l'ovest nord-ovest; ma non ha avuto questa la forza di rompere li fianchi, o le pareti del volcano, a tal che siamo nel caso di appropiarci quella memoria storica. MARCO ÆMILIO C. AURELIO Coss. *Ætna mons terræmotu, ignes super verticem late diffudit.* Jul. Obsequ. de Prodig. c. 89.

II. *Nova experimenta Chemica quæ ad penitiorem Acidi e Pinguedine eruti cognitionem valere videntur. Scribebat* D. Laurentius Crellius, Gulielmo Huntero, *M. D. S. R. S.*

Read November 15, 1781.

NON ſine maxima animi voluptate e Philoſophicarum Tranſactionum volumine noviſſimo percepi, meas litteras ad te miſſas, de experimentis chemicis referentes, non ſolum tibi haud diſplicuiſſe; ſed etiam illuſtriſſimæ Regiæ Scientiarum Societati abs te, ea qua es humanitate oblatas, honorificentiſſimis illius ſuffragiis eſſe ornatas. Quanti hunc inſignem in me collatum honorem faciam, quanta ſit mea in celeberrimos viros, ſumma benignitate de commentariolo meo qualicunque judicantes, reverentia, quantæ denique grates, quas tibi, vir celeberrime, debeam, non poſſum verbis ſatis demonſtrare. Perſuaſum itaque te habeas, oro rogoque, de mente mea tibi devinctiſſima; quam ut quoque, omni qua polleo, facultate teſter, ill. Reg. Soc. ſubjunxi huic epiſtolæ nova ea experimenta chemica, quæ ad penitiorem acidi e pinguedine eruti cognitionem valere mihi videbantur; ea ſpe fretus, fore, ut hujuſce diſquiſitionis chemicæ partem poſteriorem judicans non inferiorem priori, humaniſſime illam offeras meo nomine Sociis celeberrimis, in ſummæ meæ in illos reverentiæ documentum.

Propoſui ut noſti, vir celeberrime, modum iſtum, acidum pinguedinis concentratum acidi vitriolici ope obtinendi, quod nimirum, affuſum ſali ſegneriano ex hac partem acidam expellit

expellit forma vaporum. Quæ autem ne obnoxia ſit objectioni, quod huic vitrioli oleum ſit admixtum, et ut cognoſcerem, quomodo ſe habeat ſal medium noſtrum, igne uſtum, huic illud tradidi.

EXP. LVI.

Tres nimirum ſalis ſegneriani (ſeu ex noſtro, e pinguedine deſtillata eruto acido, et ſale alcalino vegetabili conflati) uncias, retortæ vitreæ loricatæ inditas, igni aperto per gradus aucto, expoſui. Deſtillabat in initio in excipulum aquoſi quid (cryſtalliſationis nimirum aqua). Increſcente calore, ita ut retorta ignire inciperet, protinus ſurgebant vapores copioſiſſimi griſei, acidum forte, ut mihi quidem videbatur, præſagientes: ſed vaſibus frigefactis, et apertis nihil fumi percipiebam, nec odorem acidi ſuetum; ſed potius illum ſpiritus tartari, cui fluidum quoque obtentum (ponderis drachmarum XI.) in ceteris qualitatibus, e. g. ſapore, colore aureo, ſimile erat; cum ſale tartari, parum efferveſcens. Reſiduum ſal erat alcalinum, carbonacei modo quid continens; ſed alcalini volatilis ne veſtigium quidem prodens. Sicco jam præteribo pede, ſingularem acidi fortis (exp. 53.) VI ignis mutationem in mite, quæ etiam in terra foliata tartari, ſale acetoſellæ et tartaro ipſo obtinet, et a deſtructione quadam acidi, phlogiſto intime mixti pendere videtur, niſi forſan hoc acidum (ut cl. PRIESTLEY celeberr. collega tuus, illuſtris HUNTERE, in clyſſi nitri præparatione contendit) in aeris ſingularis ſpeciem tranſeat.

Modus acidum pinguedinis fumans obtinendi mihi huc uſque laboris et tædii plenus fuerat: peractis nimirum novem deſtillationibus (exp. 1 – 9.) et rectificatione (exp. 46.) acidum erat ſaturandum ſale alcalino, quod evaporandum, calcinandum, iterum ſolvendum et inſpiſſandum, priuſquam vitrioli oleum, purum ex eo acidum expelleret (exp. 53.). Eundem nunc

finem obtinendi methodo in compendium redacta, mihi in votis erat; quod, non ſine ſpe quadam ſequenti modo conficere tentabam.

EXP. LVII.

Impoſui veſicæ et alembico, cupreis, ſtanno intus obductis, ſebum depuratum, leni igne ſubſtrato nil niſi aquam emittens, quam autem, illo adaucto, ſequebatur fluidum virideſcens. At eodem tempore ſtannum variis in alembici locis, præcipue in tubo huic appoſito, fundebatur et in externam ſuperficiem penetrabat. Finita deſtillatione, in excipulo inveni acidum et oleum, citra exſpectationem meam, utrumque fluidum, nec ut antea coagulatum, quamvis reſiduum totum fere in carbonem verſum eſſet. Hac quidem ratione laborum compendium quoddam repereram, a repetendis deſtillationibus et fuſionibus abſolutus; acidum vero non cupro ſolum inquinatum erat; ſed vaſa etiam ignis vi ita læſa erant, ut nonniſi magna adhibita opera, aliis laboribus inſervire iterum poſſent.

Spreta itaque hac methodo, votis non ex omni parte reſpondente, alia occurrit, periculum videlicet inſtituendi cum ſolutione ſebi in ſale alcalino, ſeu cum ſapone. Veriſimile enim mihi videbatur, illud dum ſolveret adipem, acidum præcipue, in hoc contentum, eſſe arrepturum: quo facto, ſi oleum ſaponis poſſet ſeparari a ſale ſegneriano, tunc ſtatim ad illud ſtadium proceſſus pervenirem, quod haud ſine mora, modo in exp. 46. eram aſſecutus: quæ autem ſeparatio facillima mihi videbatur, quia ſapo a quovis acido, nec non ſalibus mediis quibuſdam, in partes dirimitur: quo deſtructo itaque, oleum filtro ſeparare a fluido aquoſo, hoc evaporare, tunc addere vitrioli acidum, mens mihi erat. Hæc ponderans, percepi ſaponem communem non poſſe adhiberi, quia tum lixivium ex cineribus paratum varia

ſalia.

ſalia media contineat, cum ſal culinare adhibeatur ad ſaponem ex aqua ſeparandum, quod pro parte huic ſe jungit. Saponis ita ſpeciem mihimet ipſi confeci.

EXP. LVIII.

Calcis vivæ recentis libram dimidiam itaque bene obrui cum ſalis tartari libra una, et linteo leviter tecta tam diu repoſui, donec calx findi et dehiſcere incipiebat. Tunc adfudi aquæ calidæ libras ſex, quæ coctæ in vaſe ferreo ad quartæ partis conſumtionem, per linteum denſum tranſcolui (quod autem lixivium nunc ovum recens ſuſtinebat). Quartam hujus partem, aqua tantiſper dilutam coxi cum ſebi libra una, donec maxima humiditatis parte evaporata, quam optime inter ſe coire inciperent. Affuſa nunc lixivii reliqua parte coxi leniſſimo igne, continuata agitatione, uſque dum mixtura pellucida et quaſi mucilaginoſa adparebat, et frigefacta gelatinæ inſtar concreſcebat, ſaponi communi, antequam ſal culinare adjicitur perfecte ſimilis. Separando nunc iterum oleo, a ſale alcalino nihil mihi magis idoneum videbatur, alumine, quia partim minimi conſtet, partim non timendum eſſet, illud via humida, quod ab acidis mineralibus expectare mihi fas fuiſſet, ſalem ſegnerianum eſſe deſtructurum, quo facto acidum pinguedinis in auram abiiſſet; aluminis vero acidum tantum modo terræ ſuæ ipſius actionem infringentis deponit, quantum ſalis alcalini liberi invenit.

EXP. LIX.

Gelatinæ itaque exp. anteced. in aqua ſolutæ injeci alumen pulveriſatum, quod eodem momento vi quadam oleum coactum in ſuperficiem urgebat. Hoc per cochleat cribratum ſublato, iterum alumen adjeci; atque eodem modo perrexi uſque dum poſt novam ejus additionem, nihil coacti ſuperficiem

occuparet amplius*. Fluidum colatum (ut terra aluminis, et particulæ quædam olei coacti separarentur) ad siccitatem evaporavi †.

EXP. LX.

Ut acidum, quod exspectabam, ex sale segneriano expellerem, alumen, adhibere mihi occurrebat, ut eo certius acidum illud a vitriolico liberum obtinerem. Quamobrem addebam 2. partibus salis 1. partem aluminis usti, quas igni fortiori balnei arenæ exponebam. Peracta destillatione in excipulo inveni acidum fumans, ejusdem naturæ ut illud exp. 53. eoque modo finem concentrandi processus obtinuisse lætus perspexi. Attamen illi acido animadverti odorem quemdam sulphureum esse admixtum; et licet cl. BEAUMÉ (Chym. exp. t. 1. p. 335.) affirmet, parum acidi ex alumine expelli vi ignis solius; tamen a vero propius abesse in hoc processu mihi videbatur partes oleosas, massæ nostræ salinæ adhuc adhærentes, separationem acidi a terra aluminosa promovisse. Qua re motus, potius oleum vitrioli adhibere decrevi, quia tum ignis gradum poscat minorem, cum tota massa salina minor fiat.

EXP. LXI.

Tribus partibus massæ nostræ salinæ ‡ affudi unam olei vi-

* Reiteratis periculis hanc erui partium proportionem. Libris x. gelatinæ, in aqua solvendæ, addantur successive aluminis unc. xxii. (quarum aqua crystallisationis erit circa unc. xi. terra aluminosa oz. iv. cum dimidia). Mixtio hæcce, colata, evaporata, dat salis unc. xxi. cum dimidia, quod ex tartaro vitriolato, sale segneriano et aluminis haud destructi parte compositum est.

† Si fluidum illud ad crystallisandum reponere placet, adhibito studio, tartarus vitriolatus, et superfluum alumen quoad maximam partem inde separari, et quod reliquum est, inspissari tunc potest: quo facto massa salina valde imminuitur.

‡ Optima proportio hæc est: salis nostri (exp. lix. not. *) ½ adduntur, olei vitrioli unc. iv. cum dimidia; acidum transtillatum adjicitur reliquæ massæ ½, ut hoc modo rectificetur. Tunc habebis circiter unc. v. acidi limpidi fumantis.

trioli,

trioli, quod ſtatim vapores griſeos extricabat, acidum pinguedinis redolentes. Minor caloris gradus ſufficiebat omni expellendo acido: nam et maximus nihil amplius educebat præter quaſdam guttas olei ex bruno rubri *.

Ut cognoſcerem, an vitriolico acido noſtrum ſit inquinatum, partem ejus quandam ſuperfudi ſoluto ſaturni ſaccharo, qua metallum exturbatum non iterum ſolvebatur adjecto vini aceto, quamvis illud digeſtione et ipſa coctione ſedimenti tentaverim †. Detecto itaque acido vitriolico, ſeparandum illud eſſe ab acido noſtro putavi, ſi nimirum, novæ maſſæ ſalinæ adjectum, iterum evocaretur: qua videlicet methodo acidum vitriolicum ſali alcalino nubens, pinguedinis acidum expelleret ‡.

* Notatu dignum eſt, quod quavis via poſt evocatum acidum obtinerem aliquid ſalis ammoniaci animalis ficci, quod ſcilicet in aqua ſolutum ſenſum frigoris excitabat, cum calce viva tritum odorem ſpargebat alcali volatilis, et cum acido vitrioli, illum acidi pinguedinis. Quod vero ſal alcalinum volatile, cum non ineſſe videbatur adipi, ut lateret in ſale tartari (cf. cl. WIEGLEB. de ſalibus alcalinis) vel ut hoc, ope olei, volatile ſit redditum (de quo autem dubito) neceſſe eſt.

† Cl. RETZIUS in Act. Acad. Stockh. t. 32. p. 216. contendit, certum hoc eſſe indicium abſentis acidi vitrioli, ſi ſedimentum ex ſaturni ſaccharo ſoluto per adjectum acidum ortum, reſolveretur addito nitri acido; ſed contrarium expertus ſum. Adjectis enim drachmæ I. acidi noſtri, guttis olei vitrioli quatuor, ſedimentum quidem in ſaturni ſaccharo ſoluto oriebatur; non ſtatim ſolvendum per nitri ſpiritum, ſed decantato a ſedimento fluido, et adjecta nova nitroſi acido portione, hoc utique diſparebat. Quod ſi vero eodem menſtruo plumbum præcipitabatur, hoc per unc. IV. aceti vini nullo modo rurſus diſſolvere valebam, quamvis drachm. V—VI. ſufficerent diſſolvendo ſedimento, abſque guttis IV. acidi vitriolici exorto.

‡ Hac methodo in uſum vocata, etiam uti licet cineribus clavellatis ad conficiendum ſaponem noſtrum; nam rectificando acidum noſtrum ſuper nova maſſæ ſalinæ quantitate, quodlibet acidum minerale illo pinguedinis admixtum, in illa maſſa remanet.

EXP-

EXP. LXII.

Acidi itaque noſtri unc. IV. novæ maſſæ ſalinæ unc. I. adjectas leni igni expoſui; quo facto in excipulum tranſtillabat acidum fumans, coloris limpidi, quod ſaturni ſaccharo ſoluto admixtum ſedimentum quidem producebat, ad reſolvendum adjecto vini aceto.

EXP. LXIII.

Concentrati acidi noſtri vim in metalla experiundi nunc animus erat; illudque applicandi auro, quamvis ſpei haud multum rationes theoreticæ injicerent, fore ut ſolveretur. Auri itaque, ferri vitriolo præcipitati, granis IV. affudi acidi unc. I. quod leni calore digeſtum limpidum colorem in aureum mutaverat; magna licet metalli quantitate in fundo adhuc remanente; cujus vero coloris rationem peregrino cuidam, auro forte adhærenti, potius tribuens; idem cum hujus foliolis periclitatus ſum; ſed pari eventu. Quam itaque ſolutionem apparentem ut promoverem majore calore adhibito, acidum cum foliolis retortæ parvæ indidi aliamque implevi cum eodem acido atque granis IV. platinæ, et ex utraque fluidum, coquendo, evocavi, quod rurſus reſiduis affudi et digeſſi. Color in utroque fluido aureus erat, metallorum majori quantitate licet in fundo remanente. Phænomeni haud exſpectati novitate perculſus, atque de aliis metallis ſolvendis cogitans, foliola argenti noſtro acido immiſi; quæ vero, cum diſcerperentur, fluidumque aureum colorem indueret, non potui, quin ex hoc colore, ſolutioni argenti minime convenienti in eam inciderem opinionem, illum a ſolo ipſo acido pendere.

EXP.

EXP. LXIV—LXXIV.

Unam itaque acidi limpidi unciam coquendo deſtillavi ad dimidiam; quo facto reſiduum aureo conſpicuum erat colore; finita vero deſtillatione, fluidum in excipulo tranſparens erat, in retortæ fundo vero animadvertebam circulos brunos concentricos. Fluidum ex excipulo infundebam novæ retortæ puræ, iterumque deſtillabam ad ſiccitatem, remanente pari materiæ brunæ quantitate. Eandem operationem repetii octies eodem modo; et ultima vice eandem, quam prima, repperi reſidui quantitatem; quod vero perfecte ſiccatum, in aqua plane non et in ipſo ejus acido, modo difficulter ſolvebatur; quin nitroſo acido, niſi calore adjuto, haud cederet. Acidum noſtrum fumandi vim amittebat; ejus vero acredo minime quavis deſtillatione ſic decreſcebat, ut differentia ſenſibus percipi poſſet; quæ autem ſatis conſpicua erat, ſi quod prima vice deſtillaverat, cum fluido quartæ deſtillationis, vel hæcce cum octava compareretur.

Notatu dignum utique eſt, quod hoc acidum diſtillando, vel digerendo colorem ſuum mutet, et quod antea totum volatile erat, nunc ſedimentum dimittat, et acredinis vim perdat, ita ut a vero propius abeſſe videatur, quod pertinaciori adhibita opera tandem penitus deſtruatur: qua itaque ratione medium eſſe noſtrum acidum cenſendum eſt inter acida mineralia, acetumque, eaque acida, quæ, ut tartarus et acetoſellæ ſal, ſine integra virium jactura plane tranſtillari nequeunt.

Convictus hiſce rationibus de fallaci auri in acido ſoluti augurio ex colore aureo; alia inſtitui hac de re experimenta.

EXP. LXXV.

Auri puri bracteolam, et platinæ granula quædam in vasis bene occlusis per 6. hebdomadas calori fornacis expositi: fluidumque tunc decantavi, ut adjecto sale tartari viderem, an pars quædam ex illo præcipitaretur *, quod quidem minime eveniebat. Ex hac vero mixtione calori iterum exposita, pulvis descendebat, qui, fluido decantato, aqua edulcoratus et siccatus albi coloris erat †. Quæ quidem terræ species hoc modo non solum non effervescebat: sed etiam digestione adhibita difficillime solvebatur; alia et contraria ratione intermissa nimirum siccatione se habebat; cui solutioni, an quid metallici insit, ut explorarem, tincturam sulphuris volatilem beguini (metallorum optimum proditorem) adjeci; sed præcipitatum sulphur ejusdem erat coloris, ut cum puro acido factum.

Hisce observationibus inductus pulverem illum terram meram esse opinor, quam acidum nostrum secum attollebat, quæque terrarum alcalinarum communium qualitatibus haud induta, ob volatilem naturam ad illam fluoris mineralis pertinere videtur; quod vero assertum experimentis probare, parca sedimenti quantitas vetuit.

* Mirum forsan posset videri, quare solutionem stanni in aqua regia non adjecissem: negare quoque non possum, me ab illa in periculo quodam adhibita, rubri coloris vestigium observasse: sed ab eadem statim abstinui, cum casu viderem, fluida supernatantia præcipitationibus (e solutionibus plumbi stanni, reguli antimonii, bismuthi et mercurii per acidum nostrum factis) confusa, invicem, novum præbere sedimentum, rubello colore conspicuum, cujus causa, ut infra patebit, in stanno latere videtur.

† Idem fere phænomenon observavi, miscendo et digerendo solutionem alcalinam, cum pinguedinis acido, quod cum argento et bismutho antea digesseram.

EXP.

EXP. LXXVI.

Auri nunc calcis per ſal tartari paratæ gr. VIII. cum acidi noſtri unc. dimidia per menſis ſpatium digeſteram, cujus tamen magna adhuc pars in fundo vaſis remanebat. Fluido colato addidi tincturam ſulphuris volatilem, quo facto mixtum colorem e cœruleo griſeum adeptum erat. Subſidentia facta, colatoque tunc fluido, reſiduum in filtro ſiccatum e nigro flavum erat, auri ſoluti præſentiam ſic demonſtrans; quod autem luculentius adhuc apparebat, evaporata parte quadam illius ſolutionis, e qua tunc cryſtalli e flavo brunæ, figuræ incertæ prodibant.

EXP. LXXVII.

Difficultatem ſolvendi auri vincere cogitabam addendo alia acida. Pari itaque aureæ calcis portioni, affundebam acidi pinguedinis guttas 40. quibus in uno vaſe addebam guttas 20. acidi nitroſi puri, in altero tantundem ſpiritus ſalis. In priori vaſe ſtatim fere conſpiciebam bullulas aereas ſeſe extricantes, atque ſolutionis initium indicantes: poſterius nullam mutationem patiebatur. Utrumque poſtea leni calore fovi; ſed licet ſolutio in priori increſceret; tamen in poſteriori nullum ejus apparebat veſtigium. Utriuſque fluidi guttas 8. infundebam in duas ſtanni ſoluti atque diluti portiones, quarum prior purpuram ſtatim dimittebat, poſterior, mutato haud colore turbidum modo aliquatenus evadebat.

EXP. LXXVIII.

Quo ex periculo cum ſpem haurirem, fore ut aurum metallicum ipſum ſolverem, ejus bracteolæ ſuperfudi guttas 80. acidi pinguedinis, et guttas 20. acidi nitroſi puri. Eodem fere momento tota ejus ſuperficies bullulis aereis tecta, placidaque erat

illius ſolutio; additis vero adhuc acidi nitroſi guttis 20.; hæcce magis, magiſque increſcebat, donec, calore adjuta, totam bracteolam conſumeret. Quod quidem phænomenon argumentum eſſe poteſt diſcriminis, acidum noſtrum inter et illud ſalis intercedentis: certo enim certius eſſe videtur, duas partes acidi ſalis fumantis, et unam aquæfortis, aurum non poſſe diſſolvere; imprimis ſi digeſtio non adhibeatur: quam ob rem itaque pinguedinis acidum ſuo jure inter efficaciora acida locum ſibi vindicare videtur.

EXP. LXXIX.

Platinæ calcem ex aqua regis per tartari ſal præcipitatam, eodem modo (exp. 76.) tractavi, cujus ſolutio colata cum beguini tinctura ſedimentum deponebat obſcurioris coloris, quod in filtro collectum, ſiccatum ex flavo brunum erat. Solutionis altera pars evaporata in cryſtallos oblongas ex flavo brunas concreſcebat; quarum copia illa, ex auro obtentas multum ſuperabat.

EXP. LXXX.

Argenti foliola ab acido quidem noſtro corroſa, parum tamen ſolvebantur; paucæ interim ejus particulæ cupro immiſſo adhærebant; et ſalis acidum affuſum, aliqualem ſed vix conſpicuam præcipitationem producebat. Argenti vero calx continuata digeſtione ſolvebatur, ex quo, adjecta tinctura beguini, metallum ſulphuri adhærens fundum petebat, quod in filtro collectum, et ſiccatum nigreſcebat. Solutio evaporata in cryſtallos coibat, albo colore (ut argentum nitratum) haud conſpicuas; id quod acido longa digeſtione obſcuriorem colorem induenti tribuo, oleum vitrioli (minime vero ſalis ſpiritus) ſolutioni admixtum, ſedimenti quid procreabat.

EXP.

EXP. LXXXI.

In mercurium noſtrum quidem acidum haud agere videbatur; ſed hoc ab illo altera vice abſtrahendo, obſervavi, metallum in pauco fluido reſiduo, mobilitatem ſuam ſolitumque ſplendorem perdidiſſe, et in maſſam quaſi cylindraceam, circumagendo vitrum, coire. Quæ quidem omnia fluido tranſtillato iterum affuſo adhuc perſtabant; admoto vero digeſtionis calore rurſus evaneſcebant. Poſtquam omne acidum ad ſiccitatem abſtractum erat, ſuperficiei retortæ majorem partem velut amalgamate obductam deprehendi, quod non a mercurii globulis, ſed a ſolidis quaſi argenteis pendere videbatur, quæ parti abſtractæ iterum affuſæ pro tempore innatabant, tandem vero ſubſidentes, ſolvebantur; id quod plus una vice obſervavi; colata ſolutio cupream laminam dealbabat; ſed illa ab adjecto ſale communi non turbabatur; mixtum vero hoc fluidum, colatum, cupro adhuc argenteum inducebat colorem.

EXP. LXXXII.

Facilius adhuc calx (a mercurio ſublimato cum tartari ſale remixto, exorta) ab acido noſtro ſuſcipiebatur abſque caloris adjumento. Quod vero mixtum (exp. 81. motus) deſtillare decrevi in arenæ balneo, cujus ſub initium fluidi aliquid tranſibat, poſtea vero paululum adaucto calore, pars quædam ſublimata alba collo retortæ adhærebat. Hæc autem nova mercurii ſublimati ſpecies, aquæ deinde immiſſa, difficilius, adhibita etiam digeſtione, ſolvebatur, et adjecto ſale tartari ſedimentum album deponebat. Cum beguini tinctura commixta ſtatim in ſubſtantiam nigram, paulo poſt in cinnabarim mutabatur; cupro attrita, exacte licet ſicca, albam ei inducebat ſuperficiem; (id quod mercurius quoque ſublimatus communis præſtat). Noſ-

trum itaque acidum, excepto ſalis acido, ſolum omnium eſt quod ſublimatum ſiccum cum mercurio facit, atque (quod ſingulare utique eſt) leniori adhuc caloris gradu, ſurgit. Arena enim inferius circumdata retorta incumbebat, receptaculo ex tenui ferri lamina confecto, atque lateribus coctis ſuperpoſito: quo modo ignis vis et ob deficientem craticulam, et ob parvum foci ſpatium magna haud eſſe poterat.

EXP. LXXXIII.

Cuprum abſque prævia digeſtione ſolvebatur, teſte viridi fluidi colore; quæ vero adhibita, illud promovebat. Evaporando quidem cryſtalli apparebant, in aere autem mox deliquescentes.

EXP. LXXXIV.

Ferri facilis ſolutio ſaporis erat adſtringentis; in cryſtallos coibat aciculares, humiditatem atmoſphæricam vix attrahentes.

EXP. LXXXV.

Plumbum difficilius ſolvitur, et potius modo corroditur, minium vero acidum facile ſubit, quod jam, ante plenariam ſolutionem, rubrum colorem exuit; albo tunc pulveri ſimile. In ſolutionis (quæ a ſale culinari haud mutatur) ſaturatæ ſuperficie oriuntur cryſtalli ad 2‴ fere longæ, pugiunculi forma præditæ, tandem deſidentes; quarum ſapor dulcis quid habet.

EXP. LXXXVI.

Regulus antimonii, abſtrahendo acidum ab illo, ſolvitur: ſi vero fluidum in excipulo illi in retorta adhuc reſiduo, affunditur, lacteum aſſumit colorem, nec pelluciditatem niſi adhibita digeſtione

digestione recuperat. Evaporata solutio in crystallos abit, in aere haud deliquescentes.

EXP. LXXXVII.

Solutio zinci facili negotio peragitur, quæ singulari sapore metallico prædita est, et adjecto sale tartari sedimentum album deponit, quod flammæ admotum (ut zinci flores) flavum evadit.

EXP. LXXXVIII.

Stanni Malaccensis rasura ab acido nostro in pulverem flavescentem corrodebatur; et majori quidem adhuc vehementia, si calori exponeretur, sic ut hujus unc. dimidia destruendis illius scrup. II. sufficeret. Odor ex mixtura surgens maxime ingratus, et illi fere similis erat, quem salis acidum cum zinco edit. Fluidum paucum supernatans turbidum erat, quod omni studio decantatum chartæ bibulæ superfudi; sed haud mutatum transibat: quin chartam istam duplicatam, immo quadruplicatam penetraret; haud minus, quam antea turbulentum. Aliquo tempore post pulvis subsidebat flavescens, atque ei supernatans, fluidum pellucidum colore pulchre roseo splendebat; quod decantare frustra tentabam; simulac enim vitrum solum modo tangebam, summa imis miscebantur; et fluidum chartam bibulam, eadem sub specie turbida penetrabat; postea iterum subsidens. Cui vero colori rubro sedimentum rubellum (exp. 75. n. *) tribuendum videtur.

Hanc corrosam calcem stanneam digerebam aqua destillata, quæ colata dein atque evaporata relinquebat sal album facile deliquescens: quod si vero eidem calci affunderem novam acidi copiam, ut eandem ex toto in fluidum roseum liquerem, illa quidem mox colore isto ornata, sed sedimenti quantitas haud imminuta erat; quæ nunc calori exposita, illud non solum non solvebat, sed et gratum colorem cum flavo commutabat.

EXP. LXXXIX.

Bismuthum, per longum quidem temporis spatium cum acido licet digestum, non solvebatur; contrarium vero eveniebat adhibita calce (quam ex solutione illius cum nitroso acido multa aqua diluto facta) dejecerat admixtum sal alcalinum. Quæ vero solutio ab aqua adjecta lacteſcebat; et sedimentum album dimittebat; ab acidis vero vitrioli et salis mutationem nullam experiebatur.

EXP. XC.

Cobalti ex smalta reductus regulus, eodem ut bismuthum modo digestus, eadem pertinacia acido resistebat: calx vero, ex nitrato cobalto per tartari sal desidens, eidem facile cedebat *. Cujus solutionis drach. tribus cum unam nitri adjicerem, et destillationem instituerem, hac ad medium perducta vapores percepi flavos, versus illius finem rubros (exp. 117). Partem salis jam crystallisati et viridis distincte observabam dealbari a vaporibus nitrosis; quod deinde solutum in aqua, speciem constituebat atramenti sic dicti sympathetici, ex flavo virideſcentis.

EXP. XCI.

In regulum niccoli (repetita sæpius, etiam adjecto carbonum pulvere, tostione, eique interposita fusione cum nitro, calce, et borace paratam) nulla fere erat acidi nostri etiam cum illo digesti actio. Adjectum ei sal tartari nihil deturbabat, quum e con-

* Hæcce solutio, calori exposita, partem aliquam dimittit, quæ postea non iterum suscipitur; id quod in aliis solutionibus c. g. niccoli et bismuthi, quoque observavi.

trario

trario tinctura beguini, parvam metalli quantitatem sulphuri unitam dejiciebat. Calx e niccolo nitrato per alcalinum salem præcipitata absque digestione dissolvebatur ab acido nostro, cujus color evadebat viridescens, et adjectis acidis vitrioli et nitri nihil dimittebat.

EXP. XCII.

Arsenicum album magna cum difficultate, digestione licet adhibita, solvebatur sic, ut ejus scrupulus unus ab acidi uncia dimidia vix susciperetur. Caloris ope autem hanc subibat major illius copia, quam eo absente sustentari poterat; quo factum ut tunc parvæ crystalli fundum peterent. Si nostr æsolutioni immittebatur cuprum, illud nihil dejiciebat, sed potius pro parte ab acido suscipiebatur. Parte aquosa sensim evaporante, sal apparebat e viridi cœruleum; sub finem vero aliud saturate viride; luculento argumento, prius esse compositum ex arsenici acido et cupro; alterum ex nostro acore, eodemque metallo.

Altera pars solutionis adjecto sale alcalino nihil dimittebat; ea quoque pars, cujus fluidum in auras abierat, adjecto oleo tartari per deliquium ex integro solvebatur; quod vero non ita multo post sedimentum deponebat, nova salis tartari portione non auferendum; idque sal neutrum arsenicale fuisse, parca aquæ quantitate haud solvendum censeo.

EXP. XCIII.

Magnesii mineram Ilfeldensem digerebam cum nostro acido, quod illam in initio corrodebat, pulveremque nigrum a crystalliformi minera separabat; deinceps vero illam in parca haud quantitate solvebat. Quod cum aliis metallis digestum acidum colorem brunum induebat, cum magnesio nullam patiebatur mutationem; et odorem spargebat ad illum solutionis stannææ acce-

accedentem; ſaporis erat metallici. Addita aqua illam turbidam paululum reddebat. Vitriolicum acidum vero nihil dejiciebat (indicio, terram calcaream mineræ haud ineſſe) cum ſale fixo alcalino mixtum; ſedimentum copioſum dimittebat, quod ſtatim novo adjecto acido iterum diſparebat; ſi vero converſa via ſal alcalinum ſedimento majori adhuc copia ſuperfunditur, illud diſſolvit; adjecto vero tunc acido, e. g. ſalis, metallicam partem iterum deponit. Tinctura beguini acidæ noſtræ ſolutioni admixta colorem præ ſe ferebat rubellum, et quæ copioſiſſime dimittebat, ſiccata, ejuſdem adhuc erant coloris.

Abſolutis nunc eis, quæ de actione acidi noſtri in metalla dicenda erant, ſupereſt adhuc, ut videamus, quam rationem habeat idem acidum metallicis ſolutionibus adjectum.

Præcipitationes metallorum, in aliis acidis ſolutorum, per acidi pinguedinis admixtionem exortæ.

EXP. XCIV.

Aurum. Hujus metalli ſolutionem in aqua regis (quam vitro, in quo medicamenta fluida aſſervari ſolent, infuderam) aeri libero expoſueram, ex qua hac ratione tandem pulchræ cryſtalli flavæ exortæ erant, quæ figuræ ſalis communis appropinquantes, ex ſuperimpoſitis lamellis angulatis conſtabant, in aere vero per plures hebdomadas detentæ haud diffluebant *. Quas quidem cryſtallos in aqua ſolvebam deſtillata ſimplici, quæ, adjecto noſtro acido, ſedimentum flavum dimittebant. Decantato fluido, illud lavabam nova aquæ quantitate, qua defuſa, cum alia ejuſdem deſtillatæ portione adjecto, ſedimen-

* Aliquo tempore poſt inveniebam haſce cryſtallos motum velut inteſtinum aſſas, volumine adjecto in farinoſam vel flocculentam ſubſtantiam mutatas.

tum

tum per plures dies digeſſi, colavi, evaporavi, atque hoc modo obtinui reſiduum, aquam ex aere attrahens.

EXP. XCV.

Platina. Ex ejus ſolutione in aqua regis acidum noſtrum dejiciebat pulverem fere aurantium, qui, edulcoratus, multa aqua ſuperfuſus, digeſtus, colato atque evaporato fluido, reſiduum exhibebat e griſeo flavidum, quod in aere auro minus diffluebat.

EXP. XCVI.

Argentum. Quod acidum pinguedinis ex argento nitrato præcipitabat *, coloris erat griſei paululum in rubellum vergentis; et, edulcoratione prægreſſa cum aqua digerebatur. Cujus parti uni addebam guttas aliquot acidi vitriolici, a quo imperfecta oriebatur præcipitatio: altera, evaporata, relinquebat reſiduum aquam valde attrahens: argentum vitriolatum ſtatim adjecto acido ſedimentum album dimittebat; luna vero cornua cum acido digeſta haud mutata videbatur.

EXP. XCVII.

Mercurius. Acidum noſtrum album producebat ſedimentum ex mercurio nitrato †. Sed (quod notatu quam maxime dignum mihi videtur) idem ex ſublimato aliquid exturbabat ‡: affuſo enim illo, mixtio paulo poſt lactea evadebat; pulverem

* Promptior adhuc erat præcipitatio per ſal ammoniacum animale.

† Hic quoque magis adhuc præcipitabatur adjecto ſale ammoniaco animali.

‡ Mercurius, acido ſalis optime nuptus, addito acido vitriolico minime mutatur; atque ſi aquæ, ſale ſelenitico fœtæ, ſublimatum ſolventes, aliquid flavi deponant, (cf. cl. BRAUNE Chym. t. II. p. 434.) quod et ipſe vidi, tribuendum mihi hoc videtur affinitati duplici, quum nimirum acidum ſalis terram calcaream quoque amet, eamque ob rationem metallum vitriolico acido cedat.

dein album deponens; quod eo citius evenit, si mixtum digeritur. Fallor; aut hoc sedimentum album ex sublimato corrosivo hac ratione ortum criterii instar esse potest ad distinguendum acidum nostrum ab aliis, præcipue vero a muriatico. Illud vero sedimentum ablutum, digestum, in aqua solvebatur, eique immissum cuprum albescebat; eadem quoque solutio evaporata residuum dabat album, in aere non liquescens.

EXP. XCVIII.

Plumbum. Quæ e saturno nitrato descendebant crystalli parvæ aciculæ formam gerentes, edulcoratæ, aqua digestæ facilius solvebantur; acidoque tunc vitriolico admixto sedimentum dimittebant. Dissipata illius solutionis humiditate superstes erat pulvis aquam parum attrahens.

EXP. XCIX.

Bismuthum. Nitrosum acidum, metallo ope digestionis solvendo destinatum, tanta aquæ copia dilutum erat, ut, solutione peracta, nova illi admixta aqua nihil præcipitaret. Simulac vero acidi nostri guttæ aliquot accedebant, pulverem album dejiciebant, qui ablutus, cum aqua digestus, solutione colata, evaporata, residuum album progignebat facillime liquescens.

EXP. C.

Regulus antimonii. Saturata ejus in aqua regis solutio, addita aqua destillata, turbida evadebat; quam colatam, nova adjecta aqua non amplius mutabat: ab affuso vero nostro acido, statim oriebatur sedimentum album, ex quo aqua extrahebat partem fluido dissipato conspicuam, quæ, humiditatem iterum attrahens, in crystallos parvas tenues coibat.

EXP. CI.

Stannum. In aqua regis ſolutum metallum dejiciebatur ab acido noſtro, et colorem induebat ex flavo brunum. Præcipitatum ablutum, et cum aqua digeſtum ſal progignebat albidum facillime liqueſcens *.

EXP. CII.

Cuprum. Hoc neque ex vitrioli cœruleo, nec ex cupro nitrato per pinguedinem præcipitabatur.

EXP. CIII.

Ferrum, nec nitratum, nec vitriolatum, cum acido noſtro mixta ſedimentum dimittebant.

EXP. CIV.

Zincum, nec nitratum, nec vitriolatum mutabantur ab acido noſtro adjecto.

EXP. CV.

Cobalti regulus, nitratus ab acido pinguedinis affuſo nullam perturbationem paſſus eſt.

EXP. CVI.

Niccoli regulo nec nitrato, nec ſalito acidum noſtrum aliquid extricare valebat.

EXP. CVII.

Arſenicum nitratum, acido noſtro admixtum, nullum ſedimentum deponebat.

* Hæc quæ ab acido noſtro natales ducunt præcipitata, nil eſſe videntur quam ſalia metallica in aqua ſolutu difficilia.

EXP. CVIII.

Magnesium nitratum nullam conversionem expertum est a nostri acidi admixtione.

Acidorum diversorum actio in sal segnerianum *.

Quod *vitriolicum* acidum, nostrum acorem ex sale hoc medio expellat; jam supra protuli.

EXP. CIX.

Nitrosum acidum. Duabus salis nostri drachmis affudo tantundem aquæ fortis, ut vocant, duplicis (quam peracta ejus præcipitatione destillaveram) nulla effervescentia sensibili inde oriente. Quod peracta destillatione excipulo inerat fluidum, saporis acidi nostri proprii erat, sed odori admixtum aliquid aquæ fortis. Decompositum vero fuisse sal nostrum, ejusque acidum expulsum, demonstrat præcipitatio celerrima e saturno nitrato, per fluidum illud, quod destillando obtinueram.

* De figura hujus salis monenda quædam adhuc mihi supersunt. Quod ad terram foliatam accedat (Exp. nov. p. 2.) asserui, Segnerum secutus, sed cum illud majori in copia paravissem, et massam salinam perscrutarem, inveni illam crustæ forma superius tectam, qua vero ablata, huic adhærebant multæ crystalli, tres ut plurimum lineas longæ, pugionis quadrangularis forma conspicuæ, quarum duo latera oppositæ ceteris angustiora erant. Si salis alcalini quantitas nimia haud adjecta est, et crystalli siccantur super charta bibula; illæ in aere haud diffluunt: qua ratione, ut et crystallorum forma, a terra foliata tartari, mirum, quantum differunt. Segnerus vero in hanc opinionem ex eo deductus videtur, quod salis medii parcam modo quantitatem pararet, quapropter latentes sub crusta salina crystallos, ob spatii et materiæ defectum, observare haud posset. Forsan nec acidum ejus ab oleosis particulis satis liberum; nec ad saturationem aliud sal alcalinum, quam cineres clavellati, adhibitum erat.

EXP.

EXP. CX.

Muriaticum acidum. Æquale noſtri ſalis medii et acidi muriatici pondus commiſcebam: quæ deſtillata, exhibebant acidi pinguedinis drachm. II. proprio odore præditas, et e ſublimato corroſivo pulverem album præcipitantes.

EXP. CXI.

Aceti vini optimi drachm. VI. ſuperfudi ſalis noſtri drachm. II. quod ex hiſce deſtillando obtinui fluidum odoris erat aceti, et, ſublimato corroſivo admixtum, illud intactum relinquebat. Cum in majorem rei explanationem reſiduo in retorta adjicerem ſalis ſpiritum, et deſtillarem, acidum, adipis ſe prodebat jam odore, et præcipitatione mercurii ſublimati.

EXP. CXII.

Fluoris acidum pari pondere noſtro ſali admixtum celerrime in illud penetrabat, ut ſiccum videretur. Quod nonniſi magno caloris gradu adhibito prodibat fluidum, fluoris acidum haud mutatum erat: quod etiam ſaturno nitrato affuſum ejus pelluciditatem (ut ei moris eſt *) non tollebat; hac ratione quam maxime abhorrens ab acido pinguedinis.

EXP. CXIII.

Phoſphori ſal. Hujus in aqua ſoluti unc. dimidiam addebam ſalis medii noſtri drachm. II. Sub initium deſtillationis fluidi quid tranſibat, quod vero nil niſi aqua erat. Qua ex excipulo evacuata, ignem adaugebam + quo adhuc aliquid ex maſſa extorquebam, quod vero nec acidum erat, nec ſaturni ſaccharum decomponebat.

* Cf. cl. SCHEELE in Comment. Stockh. vol. XXXIII.

+ Idem erat hujus gradus, qui ad ſublimandum ſal ammoniacum animale requiritur, et majorem adhuc adhibere dubitabam, cum ſal noſtrum ſolo igne vehementiori jam decomponatur.

EXP.

EXP. CXIV.

Arsenici albi et salis nostri paullisper flavidi parem quantitatem in pulverem album comminuebam, quorum actionem in se reciprocam ut promoverem, adjiciebam aquæ destillatæ drachm. II. quæ omnia leni calore digerebam. Elapsa vix horæ una quarta, pars quædam pulveris nigrescebat, et parieti in forma annuli nigri * fortiter adhærebat, reliqua massa salina ab illo separata erat. Destillando (eodem, ut in exp. præcedenti, ignis gradu) parum fluidi obtinui, quod nec sapidum erat, nec plumbum ex ejus saccharo præcipitabat. In collo retortæ parum sublimati, ejusque tenuis reperi.

EXP. CXV.

Cobaltum nitratum. Salis nostri drachm. unam immisi solutionis cobalti nitratæ unc. dimidiæ, et fluidum penitus evocavi. Sal exsiccatum in retorta viridi gaudebat colore, quod frigefactum hunc commutabat cum albo: illud solvebam in aqua destillata, quæ nunc exhibebat novam atramenti sympathetici speciem, cobaltino communi haud absimilem, in luteum modo magis vergentem colorem.

EXP. CXVI.

Salis ammoniaci animalis (ex acido pinguedinis et alcali volatili compositi) drachm. II. commiscebam cum lapidis sic dicti hæmatitæ granis XV. quæ igni exposita sublimatum exhibebant; quod vero, peracta operatione immutatum esse sal ammoniacum, reperiebam, relicto in fundo hæmatite. Eadem iterum mis-

* Nil hoc mihi videtur, nisi subita reductio quædam arsenici albi in regulum, destillatione quoque peracta nigrum adhuc erat et durum; quod abrasum, detegebat massam albam, nonnihil firmam. A vero proprius mihi esse videtur, reductionem hanc ortam esse phlogisto, sali flavido inhærente.

cebam,

cebam, addendo aquæ aliquid in meliorem utriuſque combinationem: ſed tamen eadem eveniebant omnia.

Actio acidi pinguedinis in ſalia media.

EXP. CXVII.

Nitrum. Hujus exacte depurati drachmis II. ſuperfudi acidi noſtri tantundem, quod illud cum aliqua vehementia ſolvebat. Retorta vix arenæ calidæ immiſſa, flavida jam a vaporibus evadere mihi videbatur: qui color ſemper ſaturatior fiebat, donec eundem ruborem acquireret, qui adhibito vitriolico acido conſpicitur. Fluidum in excipulo odore acido, nitroſo communi, præditum erat, cui etiam aliquid acidi pinguedinis admixtum; nam argentum purum non ſolito ab aqua forti more ſolvebatur; potius cruſta tegebatur ſatis craſſa, colore dilute hepatico conſpicua.

EXP. CXVIII.

Sal muriaticum. Hujus drachm. II. ſolvebam in acidi noſtri pari pondere. Deſtillatione ad finem tendente, diſtincte obſervabam vapores griſeos: odor fluidi in excipulo contenti, erat acidi muriatici: ſed ut hoc cum certitudine quadam conſtaret, et an acidum pinguedinis admixtum ſit, exploraretur, res haud parum perplexa erat, cum utrumque magnam inter ſe alat ſimilitudinem. Cui vero fini reſpondere ſtannum poſſe judicabam; eamque ob cauſſam miſcebam, 1. aquæ fortis guttas 80 cum guttis 40 ſpiritus ſalis; 2. eandem aquæ fortis et ſpiritus ſalis copiam (ut 1.) cum acidi pinguedinis guttis 40; 3. aquæ fortis guttas 80 cum acidi pinguedinis guttis 40. Unicuique harum mixtionum deſtinabam ſtanni malaccenſis ſcrup. II. et in quodvis vitrum, a quavis ſtanni portione, tenuiſſima fila non

prius

prius immittebam, quam olim injecta, absque caloris auxilio, plane disparuissent. Quævis enim mixtiones in metallum agebant: N. 1. maxime, N. 3. minus, N. 2. minime: cum vero N. 1. solvendo ulterius stanno impar esset, grana hujus 7. adhuc supererant: solutio erat pellucida, et absque ullo sedimento. N. 2. maxime turbidum erat, coloris e griseo flavidi; sedimento copioso nigrescente refertum; stanni residui pondus erat gran. XVII. N. 3. exhibebat solutionem pellucidam, cum parvo in brunum vergente sedimento; stanni granis 9 adhuc superstitibus. Hisce periculis peractis, quæ regulæ instar esse debere mihi animus erat, admiscebam guttis 80 fluidi ex destillatione obtenti guttas 160 ejusdem aquæ fortis. In qua mixtione dissolvebam peditentim (iisdem phænomenis apparentibus, ut in N. 1.) stanni fila, donec drachma 1. consumta esset; sed sedimenti nigri quid in fundo hærebat. Quibus ponderatis, concludendum mihi esse videtur, quod fluidum in excipulo repertum acidum fuerit muriaticum; id quod præprimis ex vaporibus griseis, et ex magna stanni copia, quæ absque magna sedimenti copia solvebatur, colligo, pinguedinis vero acidum haud admixtum fuisse; ea ex ratione censeo, quia solutio clara, nec sedimentum brunum erat; nigrum vero illud ex eo ortum fuisse judico, quod salis spiritum (concentratiorem, quam opinabar) non diluerem nitrosi acidi quantitate sufficiente.

EXP. CXIX.

Terra tartari foliata. Huic addebam æquale pondus acidi nostri, cum qua paululum efferveſcebat; peractaque tunc destillatione acetum excipulo inesse prodebat odor, nec non deficiens in sublimatum corrosivum actio.

EXP. CXX.

Sal mirabile Glauberi. Quamvis haud expectari poſſe videbatur acidum noſtrum expulſurum eſſe vitriolicum, tamen experientiam conſului. Utroque æquali pondere mixto et deſtillato, inveniebam in excipulo fluidum, quod præter odorem acidi noſtri, ſulphureum quoque admixtum habebat. Quamobrem illud affudi ſolutioni plumbi in acido noſtro factæ, quæ ſedimenti albi quid dimittebat; indicio, parvam acidi vitriolici quantitatem divulſam eſſe a ſale alcalino; quod phlogiſto acido adhuc adhærenti tribuo, quo nimirum vitriolici acidi pars volatilior reddita videtur.

EXP. CXXI.

Tartarus tartariſatus in aqua ſolutus, adjecto acido ſedimenti magni copiam dimittebat, quod fluido decantato, veri cremoris qualitates demonſtrabat.

Liceat hiſce experimentis quædam addere de ſimilitudine et cognatione acidi noſtri et muriatici. Utrumque cum alcali volatili conſtituit ſal ammoniacum ſiccum, et cum magneſia alba, ſal valde diffluens; utrumque argentum et mercurium e menſtruis præcipitat; ab utroque ſoluto regulo antimonii, adjecta aqua turbatur, et metallicam partem deponit. Eandem cognationem hoc quoque indicare videtur, quod acidum muriaticum ſolutionis argenti et mercurii in acido noſtro non præcipitet. Sed magna etiam inter utrumque intercedit differentia: noſtri nimirum acidi intima combinatio cum oleoſis partibus; ſal calcareum haud diffluens; naphthæ facilis geneſis; argenti et mercurii ſolutio via ſimplici humida; et hujus præcipitatio ex ſublimato corroſivo.

His inter ſe comparatis, characteres cognationis utriuſque acidi, quam illi diſcriminis, potiores mihi omnino eſſe videntur.

Hæc ſunt, illuſtris et celeberrime HUNTERE, pericula, quæ cum acido pinguedinis hucuſque inſtitui, omnia. Minime me fugit, materiam hanc nondum eſſe exhauſtam; multaque adhuc eſſe ſupplenda, quæ intimiori noſtri acidi cognitioni favent; imprimis explorandos eſſe affinitatis gradus, quos metalla contrahunt cum acido noſtro. Quibus vero omnibus exantlandis minime deero, ſi modo cognovero, labores meos huic rei impenſos plane haud diſplicuiſſe eruditis, imprimis illuſtriſſimæ Regiæ Societati, cujus aliquem in me favorem quam maxime cupio. Interim vale, vir celeberrime; meque tibi ut habeas commendatiſſimum, majorem in modum oro rogoque. Dabam Helmſtadii Idibus Decembr. 1780.

III. *Obſervations on the Bills of Mortality at* York. *By* William White, *M. D. F. A. S.*; *communicated by* Nathaniel Pigott, *Eſq. F. R. S.*

Read December 6, 1781.

FAITHFUL and accurate regiſters of the number of births and deaths kept in different places are of great importance to the community. The ſtateſman, the philoſopher, and the phyſician, are equally intereſted in inquiries which infallibly ſhew us the real ſtate of the nation, as to population, healthfulneſs, and, as connected with the latter, virtue and temperance.

It muſt give great pleaſure to a reflecting mind, to find, from undeniable proofs, that this nation appears to be, in the above reſpects, in a general and progreſſive ſtate of improvement. The births have become more numerous, the deaths fewer, in proportion in almoſt every place where the regiſters have been conſulted: for proof of this I refer to the Tranſactions of the Royal Society, vol. LVII. LIX. LXI. LXIV. LXV. &c. and to a publication of Mr. WALES, F. R. S. intituled, An Inquiry into the preſent State of Population in England and Wales, lately publiſhed.

It would not perhaps be difficult, and as a phyſician I could with pleaſure attempt the inveſtigation, to diſcover the various

cauſes to which ſuch effects may be attributed; but here a wide field offers itſelf to our examination. It will, however, be neceſſary juſt to point out ſuch as affect this city in particular, in a ſubſequent part of this paper.

Mr. DRAKE, F. R. S. in his Antiquities of York, has given us the number of births and burials for 7 years, from Auguſt 5, 1728 to Auguſt 5, 1735, incluſive. This gave a favourable opportunity of comparing our preſent ſtate after an elapſe of 45 years. In order to this, the different pariſh regiſters were carefully examined from January 1, 1770, to December 31, 1776, incluſive: I added the number of males and females for the latter term, which Mr. DRAKE omitted.

TABLE

TABLE I. The number of births and burials in York from Auguſt 5, 1728, to Auguſt 5, 1735.

The different pariſhes.	Births.	Burials.
All Saints, Pavement,	123	218
All Saints, North-ſtreet,	101	111
St. Crux,	132	159
St. Cuthbert's,	55	80
St. Dyonis,	92	106
St. Helen's,	113	122
St. John's,	136	173
St. Laurence,	60	77
Martin's, Conyngs-ſtreet,	73	110
Michael le Belfray,	310	327
St. Mary's, Caſtle-gate,	150	221
St. Michael, Spurrier-gate,	198	216
St. Martin's, Mickle-gate,	92	117
Biſhophill the elder,	103	117
Biſhophill the younger,	57	73
St. Maurice,	55	158
St. Margaret's	118	147
St. Olave's,	147	181
St. Saviour's,	70	103
St. Sampſon's,	188	228
Chriſt Church,	140	119
Trinity, Goodramgate,	143	144
Trinity, Mickle-gate,	129	152
Diſſenters,	18	29
	2803	3488

The burials, therefore, exceeded the births 685 in 7 years, or 98 annually.

TABLE

TABLE II. The number of births and burials from January 1, 1770, to December 31, 1776, incluſive.

The different pariſhes.	Births.	Burials.
All Saints, Pavement,	240	153
All Saints, North-ſtreet,	96	88
St. Crux,	146	109
St. Cuthbert's,	102	126
St. Dyonis,	109	96
St. Helen's,	96	76
St. John's	183	124
St. Laurence,	97	83
Martin's, Conyng-ſtreet,	104	74
Michael le Belfray,	297	298
St. Mary's, Caſtle-gate,	159	210
St. Michael's, Spurrier-gate.	151	113
Martin's, Mickle-gate,	82	98
Biſhophill the elder,	124	151
Biſhophill the younger,	121	92
St. Maurice,	76	138
St. Margaret's,	182	142
St. Olave's,	234	296
St. Saviour's,	96	108
Sampſon's,	174	184
Chriſt Church,	147	110
Trinity, Goodram-gate,	161	118
Trinity, Mickle-gate,	122	164
Diſſenters,	24	24
	3323	3175

Decreaſed in burials 313, or $44\frac{3}{7}$ annually.
Births increaſed 520, or $74\frac{2}{7}$ ditto.
Births exceed the burials 148, or $21\frac{1}{7}$, ditto.

TABLE

TABLE III. The number of births and burials, with the proportion of males and females, annually, from January 1, 1770, to December 31, 1776.

	Births.	Males.	Females.	Burials.	Males.	Females.
1770	467	237	230	417	203	214
1771	451	225	226	485	225	260
1772	490	238	252	508	220	288
1773	474	244	232	499	241	258
1774	453	214	239	382	173	209
1775	490	255	243	488	237	251
1776	498	255	243	396	177	219
	3323	1666	1657	3175	1476	1699

Number of males born in 7 years 1666, or 238 annually.
Number of males buried in 7 years 1476, or 210$\frac{6}{7}$ annually.
Number of females born in 7 years 1657, or 236$\frac{5}{7}$ annually.
Number of females buried in 7 years 1699, or 242$\frac{5}{7}$ annually.

TABLE IV. Mortality of the seasons.

Winter.		Spring.		Summer.		Autumn.	
Jan.	320	Apr.	277	July	220	Oct.	237
Feb.	282	May	265	Aug.	237	Nov.	230
Mar.	316	June	274	Sept.	225	Dec.	292
	918		816		682		759

In order to find the number of inhabitants in any place, where, either from its bulk, or other reasons, a numerical survey cannot be obtained, two methods may be made use of. The first is, multiplying the number of houses by the medium of inhabitants in each. The second is, one recommended by Monf. MOHEAN, in a work, intituled, *Recherches et Considera-*

tions ſur la Population de la France. He found, by very laborious calculations, that the number of inhabitants may be known by the births, the latter being to the former as nearly 1 to 27.

By an account given into the Houſe of Commons in March 1781, the number of houſes in York ſubject to the new houſe-tax was 2285: if to thoſe be added ſuch as were too ſmall to come under the tax, which may probably amount to one-third more, the total of the houſes in York will be about 3000. This number multiplied by 4¼, which is nearly the medium of people in a houſe, gives 12,750 for the number of inhabitants.

By the ſecond rule we have 12,798 for the number of inhatants, which is the reſult of 474, the average annual births, multiplied by 27.

The remarkable coincidence of the above methods of calculation makes it very probable, that if we eſtimate the number of inhabitants at 12,800, we ſhall not be far from the truth.

However this may be as to the exact number of inhabitants, it affects not the principal end of the preſent inquiry, which is to ſhew how we are improved in population and healthfulneſs within 40 years paſt.

In order to prove this, we muſt find the number of inhabitants in the year 1735, from tab. 1. We there find the average annual births to be 400; this multiplied by 27 gives 10,800 for the number at that time. This number divided by the average annual deaths 498, gives the proportion of deaths 1 in 21¾. Such was the ſtate of this city as to mortality 46 years ago.

Very different from this is our preſent ſituation, the proportion of deaths being now decreaſed to 1 in 28¼, which is the quotient of 12,800, the number of inhabitants divided by 453,

the present average of annual deaths. This is certainly a great rise in the scale of healthiness. From being near as fatal as London we have become less so than many country places, as will appear from the following comparative view of the proportion of deaths in different places.

At Vienna,	-	1 in 19½ dies every year
London,	-	1 in 20½
Edinburgh,		1 in 20⅝
Berlin,	-	1 in 21
Rome,	-	1 in 22
Amsterdam,		1 in 22
Dublin,	-	1 in 22
Leeds,	-	1 in 22
Northampton,		1 in 26
Shrewsbury,		1 in 26
Liverpool,		1 in 27 7/10
Manchester,		1 in 28
York,	-	1 in 28½

Hence in 1735, at York it would require 21½ years to bury a number equal to that of its inhabitants; but in 1776, 28½ years would be required for the same. One third less die yearly now than in the former period; and we are certainly advancing still higher, for in 1777 the births were more than in any former year, being 516, the burials 464.

As there is no settled manufactory here, there is little increase or decrease of the people by acquisition or emigration, and probably what may happen in either case is nearly balanced by the other.

It appears from tab. 4. that the summer season is by much the healthiest at York; autumn the next; then the spring; winter being by far the most fatal. Dr. PERCIVAL found much

the ſame to be the caſe at Manchester. At Cheſter Dr. HAYGARTH ſays November was the moſt ſickly month. The differences in the regiſters make it impoſſible to give the diſeaſes of which the individuals died; yet a general idea of this may be obtained from the ſame table. By the care and attention of the preſent archbiſhop of this province, this may be eaſily perfected in future periods.

It appears from hence, that our diſeaſes are chiefly of the inflammatory kind, which phyſicians know to be the general attendants of the winter and ſpring months. The diſorders of the ſummer and autumn are more particularly ſuch as ariſe from putreſcency and acrimony, ſuch as ſlow and remitting fevers, dyſenteries, cholera's, and the like, thoſe then being with us the healthieſt ſeaſons ſhew that we are not ſubject to putrid diſeaſes. Dr. WINTRINGHAM has given us an account of the weather and the correſponding diſeaſes at York for ſixteen years ſucceſſively, in his Commentarium Noſologicum, to which learned work I refer the curious reader for further ſatisfaction upon this ſubject.

Among the general cauſes of our increaſing population and healthineſs we may enumerate the introduction of inoculation, which has been the means of ſaving a number of lives; improvements in the treatment and cure of ſeveral diſorders, the cool regimen in fevers, the admiſſion of freſh air, the general uſe of antiſeptic medicines and diet, have doubtleſs had a ſalutary and extenſive influence upon the health of mankind, and have much obviated the malignity of ſome of our moſt dangerous diſeaſes. To theſe may be added a general improvement and greater attention to nature in the management of infants.

After

After the general cauſes of healthineſs, ſuch as are particular, or of a more local nature, come under conſideration. In this reſpect the city of York has been much improved within a few years paſt. The ſtreets have been widened in many places, by taking down a number of old houſes built in ſuch a manner as almoſt to meet in the upper ſtories, by which the ſun and air were almoſt excluded in the ſtreets and inferior apartments. They have alſo been new paved, additional drains made, and, by the preſent method of conducting the rain from the houſes, are become much drier and cleaner than formerly. The erection of the locks, about four miles below the city, has been a great advantage to it: for, before this, the river was frequently very low, leaving quantities of ſludge and dirt in the very heart of the city, alſo the filth of the common ſewers which it was unable to waſh away. The lock has effectually prevented this for the future, by the river being kept always high, broad, and ſpacious; and has thus contributed to the ſalubrity as well as beauty of York. In the above improvements, in others that are intended to take place, in the care and expence neceſſary to keep in proper repair the public walks about the city, the magiſtrates have exerted much public ſpirit, and have added to the health as well as conſulted the convenience of its inhabitants.

York,
Sept. 8, 1781.

IV. *Account of a monſtrous Birth. In a Letter from* John Torleſe, *Eſq. Chief of* Anjingo, *to the Hon.* William Hornbey, *Governor of* Bombay; *communicated by Dr.* Lind, *F. R. S.*

Read January 7, 1782.

HON. SIR, Anjingo, April 5, 1780.

AS I know you are curious with reſpect to the productions of nature, I have taken the liberty to incloſe you a drawing of a child which a Nair woman was delivered of the 28th of March at midnight, and which lived till the 1ſt of April in the morning. In the afternoon I went to ſee it in company with Mr. HUTCHENSON and Dr. CHOZIER. You will ſee by the ſketch that it had but one body, at the extremity whereof were two heads, one larger than the other. It had four hands and arms perfect, two legs on one ſide its body, and one on the other, which began on the middle of its back, and appeared by nature intended for two by its ſize and from the appearance of the foot, which looked as if two had been ſqueezed or rather maſhed together. It had but one navel and one anus, but two genitals of the female. It was fed during

Bufire sc.

its ſhort exiſtence by hand with goat's milk. It is remarkable, that one head would ſleep whilſt the other was awake; or one would cry, and the other not. They both died at the ſame inſtant. Almoſt all this town went to ſee it, the like having never been heard of before. The mother is a ſtout woman; and I ſaw four of her children at her houſe, the youngeſt of which was ſix years old, all healthy and perfect.

I am, &c.

V. *Experiments with* Chineſe Hemp Seed. *In a Letter from* Keane Fitzgerald, *Eſq. to Sir.* Joſeph Banks, *Bart. F. R. S.*

Read January 17, 1782.

SIR,

Poland Street,
Dec. 17, 1782.

EVERY thing extraordinary in art or nature falls, in ſome meaſure, within the views of the Royal Society; but how far the following account of what appeared to me an extraordinary production may be worthy of being communicated to that learned body, is ſubmitted entirely to your conſideration.

A few grains of Chineſe hemp-ſeed had been given to me by the late Mr. ELLIOT, brother to General ELLIOT, who had formerly reſided for ſome time in China. He told me, the hemp in that country was deemed ſuperior to that of any other, both for fineneſs and ſtrength, and wiſhed I would try whether it would come to maturity in this kingdom. He gave me between thirty and forty grains of ſeed for the purpoſe, which I laid by, as I thought, carefully, with intent of ſowing them the ſpring following, which is the uſual time of ſowing hemp in this country; but I had unluckily forgotten where I laid them, and did not find them till the beginning of laſt June, by which time I imagined them to be very unfit for vegetation; but as I concluded they would be ſtill more ſo by keeping them till the ſucceeding April, I had them ſowed the 4th day of that month, and was much ſurpriſed to find that thirty-two

of

of the ſeeds had vegetated ſtrongly, and grown to an amazing ſize, ſeveral of the plants meaſuring in height more than fourteen feet, and ſeven inches nearly in circumference, by the middle of October following, at which time they came into bloom. There were from thirty to forty lateral branches on a plant; theſe were ſet off in pairs, one on each ſide of the ſtem pointing horizontally; the others at about five or ſix inches diſtance from them, pointing in different directions, and ſo on to the top, the bottom branches of ſome meaſuring more than five feet, the others decreaſing gradually in length towards the top, ſo as to form a beautiful cone when in flower, which were unluckily nipped by a few nights froſt that happened to be pretty ſharp towards the end of the month; and the plants began to droop at the beginning of November at which time I had them pulled up by the roots.

As I was but little acquainted either with the cultivation of the ſeed, or preparing the plants afterwards for the production of hemp, and as theſe plants were very different in their ſize from any I had ever ſeen, the beſt method that occurred to me was, that of ſteeping them in water, where I let them remain for a fortnight, and then placed them in an upright poſition againſt a ſouth wall to dry and bleach.

On trying whether the hemp could be eaſily ſeparated from the woody part, I was agreeably ſurpriſed to find, that on peeling a few inches longitudinally from the root, the whole rind, from the bottom to the top, not only of the ſtem but alſo of all the lateral branches, ſtripped off cleanly, without breaking any one of them. The toughneſs of the hemp ſeemed to be extraordinary, and upon drying and beating divides into an infinity of tough fibres. The plants when ſtripped are quite white, and when the lateral branches are cut off, appear

like handſome young poles. They are perforated in the middle, but the perforation is not larger than that of a goose quill, in a ſtem of more than two inches diameter. The woody part ſeems pretty ſubſtantial, and if they ſhould be found of any duration, might be applied to many uſeful purpoſes; or if not, I ſhould imagine they would produce plenty of good aſhes by burning.

The rough hemp that has been pealed from the thirty-two plants, when thoroughly dried, weighed three pounds and a quarter; but I do not think it had come to full maturity, though I can hardly doubt but the plants would have come to perfection if the ſeed had been ſown in the proper ſeaſon. The ſummer was remarkably dry, notwithſtanding which, although the ſituation they were placed in was very warm, and the ground not rich, I found, on meaſuring the plants at different times, that they had grown nearly eleven inches *per* week.

As the culture of ſo valuable a kind of hemp as this promiſes to produce appears to be of conſequence to a maritime and commercial kingdom, I have applied to the Directors of the Eaſt India Company, to give proper orders to their factors and ſuper-cargoes in China, to procure ſome of the beſt ſeed that can be obtained; and ſend, even a ſmall parcel, by each of their returning ſhips, which they have very obligingly promiſed; and from what has already appeared, there can be no doubt of its continuing in a ſtate fit for vegetation for a much longer time than is uſually required for that voyage.

If the ſeed ſhould arrive in ſafety, I can hardly doubt of obtaining the aſſiſtance of the Society eſtabliſhed for the Encouragement of Arts, Manufactures, and Commerce; and ſhould expect, from their wonted aſſiduity and liberal diſpoſition of

proper

proper rewards for the culture and manufacture of ſo valuable a commodity, to ſee it as ſucceſsfully carried to perfection as ſeveral other branches have happily attained by their care and protection; and ſhall think myſelf very happy in being any ways inſtrumental in forwarding ſo good a purpoſe.

As you did me the favour of examining the plants in a growing ſtate, I need not trouble you with any ſpecimens from them; but if they ſhould be deemed worthy of being laid before the Society, I ſhall ſend ſome of the hemp in the ſtate it was peeled, and a piece of the ſtem it was peeled from, as alſo ſpecimens of the leaf and flower, for their examination.

I am, &c.

VI. *An Account of ſome Scoria from Iron Works, which reſemble the vitrified Filaments deſcribed by Sir* William Hamilton. *In a Letter from* Samuel More, *Eſq. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 17, 1781.

SIR,

IN the very accurate account given of the eruption of Mount Veſuvius in the month of Auguſt, 1779, in a letter from Sir WILLIAM HAMILTON, printed in the Philoſophical Tranſactions, vol. LXX. part I. p. 42. et ſeq. among many other equally curious informations, it is ſaid, "Long Filaments of "vitrified matter, like ſpun-glaſs, were mixed with and fell with the aſhes." And in a note annexed it is alſo ſaid, that "during an eruption of the volcano in the Iſle of Bourbon in "1766, ſome miles of country, at the diſtance of ſix leagues "from the volcano, were covered with a flexible capillary yel-"low glaſs, ſome of which were two or three feet long, with "ſmall vitrous globules at a little diſtance one from the other."

There appeared to me on reading theſe paſſages an exact ſimilarity between theſe productions of the two volcanos and ſome ſcoria I had received from a worthy friend, who is maſter of one of the largeſt works in England for ſmelting iron. In a letter accompanying the ſpecimen, he writes, "I have ſent "a ſpecimen of ſome ſlag, or vitrified cinder, which has by

" the reverberation of the blaſt from the Tweer *, been drawn " out whilſt fluid into long cobweb-like threads (ſometimes " ten or twelve feet in length) and affixed itſelf to the beams, " &c. of the bellows room."

Whoever has attentively viewed the large furnaces wherein iron ore is ſmelted by coak, will readily allow, that they preſent the moſt ſtriking reſemblance (however diminiſhed) of that moſt tremendous of all appearances, the eruption of a volcano; and that the moſt exact pictures hitherto ſeen of the flowing of the lava from the one is ſhewn by the running of the ſlag from the other: this has induced me to lay before you, for the inſpection of the Royal Society if you judge it worthy their attention, ſome of the ſcoria in its capillary ſtate, and with all due deference to the acknowledged abilities of Sir WILLIAM HAMILTON, to ſubmit to your conſideration, and that of the learned Body over which you ſo deſervedly preſide, whether the fine filaments may not be produced in the eruption of the great furnaces of nature, by means ſimilar to thoſe by which we ſee them formed in the furnaces of art. Sir WILLIAM ſeems to think, " That (what he calls) the natural ſpun glaſs which " fell at Ottaiano, as well as that which fell in the Iſle of " Bourbon in 1766, muſt have been formed, moſt probably, " by the operation of ſuch a ſort of lava as has been juſt de- " ſcribed (that is, perfectly vitrified) cracking, and ſeparating " in the air at the time of its emiſſion from the volcanos, and " by that means ſpinning out the pure vitrified matter from " its pores or cells, the wind at the ſame time carrying off thoſe " filaments of glaſs as faſt as they were produced." See p. 81.

* The Tweer is that opening through which the air is driven by the bellows into the body of the furnace.

That ſome of the fine filaments found after the eruptions of the volcanos were formed in this manner is not unlikely: but as we ſee about the iron furnaces the vitrified ſcoria drawn into fine threads, of very conſiderable length, by the ſimple action of the wind from the bellows, is it not very probable, that the far greater part at leaſt of thoſe filaments ſcattered over the land, and which were found two or three feet long, were drawn out before the ejection of the lava from the crater by the force of thoſe violent torrents of wind which muſt be required to ſupport and actuate ſo intenſe a fire as at thoſe times fills the body of the mountain?.

In all matters of this kind there is great ſcope for conjecture, and much muſt be allowed to it; and I have preſumed to ſubmit this opinion to you, not with an intention to diſpute the probability of what has been already advanced on this head, but to point out from what occurs immediately under the eye of every workman about our iron furnaces, ſome eaſy and ſimple mode of accounting for ſo ſingular a phenomenon, and as an introduction to my preſenting to the Royal Society a ſpecimen of ſo curious a production.

The extreme fineneſs to which theſe filaments are reduced, and their brittleneſs, render it almoſt impoſſible to convey them to any diſtance, preſerving at the ſame time any conſiderable length of the fibres; theſe which I have now the honour to lay before you reſemble cotton in appearance, but if examined with a microſcope will be found in all reſpects ſimilar to thoſe deſcribed by Sir WILLIAM HAMILTON.

I am, &c.

VII. *An Extract of the Regiſter of the Pariſh of* Holy Croſs, Salop, *being a Third Decade of Years from* Michaelmas 1770 *to* Michaelmas 1780, *carefully digeſted in the following table. By the Rev. Mr.* William Gorſuch, *Vicar; communicated by Dr.* Price, *F. R. S.*

Read January 17, 1781.

		1771	1772	1773	1774	1775	1776	1777	1778	1779	1780		
Baptized	Males	23	20	19	20	18	31	16	18	20	18	203	385
	Females	16	18	16	12	23	14	17	27	22	17	182	
Buried	Males	16	19	12	11	13	28	13	12	23	13	160	311
	Females	13	20	17	10	6	21	14	15	13	22	151	
											Increaſe	74	

	1771		1772		1773		1774		1775		1776		1777		1778		1779		1780		Total	
	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F
Under a month	0	1	—	—	1	1	2	1	1	0	2	2	—	—	1	1	4	3	0	2	11	11
From mo. to 1 yr.	2	3	2	2	0	3	0	3	0	1	2	2	2	1	2	2	4	1	1	5	15	23
1 to 2	1	0	1	3	2	3	—	—	0	1	4	4	—	—	—	—	2	0	1	0	11	11
2 to 5	1	0	4	2	1	2	—	—	1	0	5	7	2	3	2	1	1	0	—	—	17	15
5 to 10	—	—	2	2	0	1	—	—	0	1	3	1	0	2	2	0	—	—	—	—	7	7
10 to 15	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0	2	1	0	1	2
15 to 20	—	1	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	2
20 to 25	1	0	—	—	—	—	—	—	0	1	1	0	—	—	1	1	1	0	—	—	4	2
25 to 30	—	1	0	—	—	—	—	—	3	0	1	1	1	1	0	1	0	1	1	1	8	4
30 to 35	—	—	—	—	1	2	1	0	—	—	—	—	0	1	0	1	1	0	—	—	3	4
35 to 40	2	0	—	—	—	—	1	2	2	0	1	1	—	—	1	0	—	—	1	0	8	3
40 to 45	2	2	0	1	1	0	1	0	1	0	—	—	—	—	—	—	0	1	2	0	7	4
45 to 50	2	0	1	1	0	2	—	—	—	—	—	—	0	1	0	1	—	—	—	—	3	5
50 to 55	1	0	1	1	1	1	1	2	1	0	1	1	2	0	0	1	3	1	0	1	11	8
55 to 60	2	1	—	—	—	—	—	—	1	0	—	—	1	0	1	0	—	—	0	3	5	4
60 to 65	0	1	0	1	0	1	2	0	1	0	3	0	1	2	0	2	3	0	3	2	13	9
65 to 70	—	1	0	—	—	—	—	—	—	—	—	—	—	—	2	2	—	—	0	1	3	3
70 to 75	0	2	1	3	4	0	1	1	1	2	2	0	—	—	0	2	0	2	2	3	11	15
75 to 80	2	1	1	1	—	—	—	—	1	0	2	0	0	4	—	—	2	1	1	3	9	7
80 to 85	0	2	2	1	1	0	2	0	—	—	1	2	4	1	—	—	2	1	—	—	12	7
85 to 90	—	1	0	—	—	—	—	—	—	—	—	—	0	1	—	—	—	—	—	—	1	1
90 to 95	—	—	—	—	0	1	0	1	0	1	—	—	—	—	—	—	—	—	0	1	0	4

An

An actual ſurvey was made in 1775, when the number of the inhabitants was found to be total 1057: of which under ten 287, and above ſeventy 57, *viz.* from 70 to 75, males 12 females 10 = 22. From 75 to 80, males 8 females 11 = 19. From 80 to 85, males 8 females 6 = 14. From 85 to 90, males 1 females 1 = 2.

An actual ſurvey was made in the year 1780, when the number of inhabitants were 1113.

There remains alive in 1780.

Under ten years of age,	males 155	} 293	
	females 138		
From 70 to 75	males 6	} 17	
	females 11		
From 75 to 80	males 5	} 13	
	females 8		
From 80 to 85	males 2	} 6	
	females 4		
From 85 to 89	males 2	} 3	
	females 1		

Diſtempers

Diſtempers and Caſualties from 1770 to 1780.

Accidents	6	Meazles	7
Apoplexy	5	Palſy	9
Aſtma	5	Pleureſy	1
Cancer	2	Rheumatiſm	1
Chin-cough	5	Small-pox	43
Conſumption	62	Sore-throat	8
Child-bed	3	Stone	2
Convulſions	23	Suddenly	2
Dropſy	20	Teeth	2
Drowned	3	Untimely	4
Fever	15	Worms	3
Jaundice	2		
Lues venerea	1	The remainder died of a natural decay, without any oſtenſible Diſtemper.	
Mortification local	5		
Mortification inteſtine	10		

The number of inhabitants actually ſurveyed every five years for thirty years.

In 1755	1049
1760	1048
1765	1096
1770	1046
1775	1057
1780	1113

The increaſe of 48 perſons in the year 1765 was owing to the ingreſs of four numerous families into large houſes, which were almoſt uninhabited for many years before.

The

The decreaſe of 50 perſons in the year 1770 was occaſioned by the demoliſhing of nine houſes, in order to open a way to the new ſtone bridge built over the river Severn.

On Good Friday, 1774, there happened a dreadful fire which originated from a chimney, and extended on both ſides of the ſtreet to the diſtance of half a mile: the wind blowing with great violence, the flames in a few hours conſumed 48 houſes, being generally thatched buildings. In this conflagration 179 of the inhabitants loſt their dwellings, but immediately provided themſelves with lodgings within the pariſh, and of the number of ſufferers only 24 perſons went out of the pariſh, and returned no more. The ground vacated by the houſes burnt is now, in 1780, built upon, and moſtly covered with little tenements fitted for poor inhabitants to live in, and made ſo commodious as to receive a greater number of inhabitants than they did before the fire in 1774; ſo that the families, whoſe number in 1770 were 240, are in the year 1780 increaſed to 246 nearly, and perhaps will be increaſing when more buildings ſhall be erected.

Houſes pay window lights 65. The new houſe tax paid by 36 houſes. The firſt decade was publiſhed in the Phil. Tranſ. vol LII. part I. art. 25. The ſecond decade was publiſhed in the Phil. Tranſ. vol. LXI. art. 6. p. 57. See alſo Dr. PRICE's Obſervations on Reverſionary Payments, ed. 1771. p. 192. and note *a*. alſo p. 259. and 263.

The taking account of the marriages in this pariſh cannot be of any uſe in political arithmetic, becauſe it is the cuſtom of the fixed inhabitants to go out of the pariſh, and be married in diſtant churches; and the weddings performed in this church are generally between ſtrangers who occaſionally reſide here ſo long as to make a place of abode according to the act

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of parliament made in 1754. Dr. HEBERDEN hath made a very proper use of the number and proportion of marriages in the island of Madeira; but then it was an island, and all are confined to their own constant residence. If the whole island of Great Britain was universally to be included in the account of the number of marriages, it would be very useful and compleat.

Many young people have gone out of this parish to supply the navy and army, but probably the same number would have emigrated, to be apprenticed and follow different occupations.

The public register is too general for the purposes of political arithmetic. The extracts here made are drawn from private papers, kept with great care and exactness, so that the births and burials of the fixed inhabitants are not rendered incorrect by the accidental ingress of foreigners, or temporary egress of the real inhabitants.

VIII. *An Experiment propoſed for determining, by the Aberration of the fixed Stars, whether the Rays of Light, in pervading different Media, change their Velocity according to the Law which reſults from* Sir Iſaac Newton's *Ideas concerning the Cauſe of Refraction; and for aſcertaining their Velocity in every Medium whoſe refractive Denſity is known.* By Patrick Wilſon, *A. M. Aſſiſtant to* Alexander Wilſon, *M. D. Profeſſor of Practical Aſtronomy in the Univerſity of* Glaſgow; *communicated by the Rev.* Nevil Maſkelyne, *D. D. F. R. S. Aſtronomer Royal.*

Read January 24, 1782.

UPON the ſuppoſition that the refraction of light is cauſed by a certain action of groſs and ſenſible bodies upon it, Sir ISAAC NEWTON has demonſtrated, that the ſines of incidence and refraction, when the rays paſs out of one medium into another of different denſity, muſt always be in a conſtant ratio. This conſtancy of the ratio of the ſines is agreeable to an univerſal experience, and has been called the law of refraction. Upon the ſame grounds he has alſo demonſtrated, that the velocity of the rays muſt be greater in the more refracting medium in the inverſe ratio of the ſines. Of this property of refraction, however, we have hitherto had no evidence in the way of experiment. The ideas entertained by Sir ISAAC NEWTON, from which this property has been deduced, though they confeſs their great author, by a moſt beautiful ſimplicity,

and

and by a very ſtriking agreement with fact, have yet been deemed by ſome perſons as not perfectly authentic. His contemporary LEIBNITZ and others have attempted demonſtrations of the law of refraction from principles very different, and which do not lead to the opinion of the acceleration of light in the more refracting medium. At preſent it is propoſed to point out a method of determining experimentally the law of the variation of the velocity of light, according to the change of the medium. If obſervations ſhall ſhew this law to be agreeable to Sir ISAAC NEWTON's concluſions, we ſhall then have a very ſtrong additional evidence in favour of his principles. If, contrary to the moſt probable iſſue of the experiment, ſome unſuſpected law ſhould be diſcovered, we muſt, according to the rules of induction laid down by that great maſter in philoſophy, ſo far reſtrict our general concluſions, and accommodate our ideas to the real condition of things.

The method of experiment at preſent alluded to is, that of obſerving the aberration of the fixed ſtars with a teleſcope filled with a denſe fluid, ſuch as water, or any other equally limpid and of greater refraction, fitted to bring the rays to a focus by the ſurface of the medium oppoſed to the object having a proper degree of convexity. It is enough at this time to ſuggeſt a general notion of the inſtrument, and we now proceed to explain in what manner it can aſſiſt us in the preſent inquiry.

Since aberration, taken in its enlarged ſenſe, depends on the relative velocities of light and of the teleſcope, if the rays were really to move much faſter or much ſlower in an unuſual teleſcope of this kind, it ſeems to follow, that the quantity of aberration given in theſe circumſtances, compared with Dr. BRADLEY's angle, would certainly indicate the new rate of velocity. Such an inference would certainly be juſt, and it is

upon theſe grounds that we propoſe to inquire into the velocity of the rays, as they move forward in denſe media ſo applied to teleſcopes. Granting, however, for the ſake of argument, that light moves down through ſuch an unuſual teleſcope with an increaſed velocity ſuited to the refractive denſity of the medium, it will by no means happen, that the aberration will be changed on that account. This propoſition, which at firſt view may appear paradoxical, and even contradictory to what has been affirmed above, is however not the leſs certain, and may ſerve to ſhew, what caution is ſometimes requiſite in applying general principles to particular caſes: for it ſhall be proved, that the aberration in ſuch a teleſcope will preciſely agree with that of Dr. BRADLEY's only in the caſe of the rays moving ſwifter in the watery medium than in air, in the ratio aſſigned by Sir ISAAC NEWTON, and that this ſameneſs of aberration will itſelf be a proof of light being ſo accelerated within the teleſcope.

In the illuſtrations which follow, the reader is ſuppoſed not to be wholly unaccuſtomed to the diſtinctions betwixt abſolute and relative motion, as this will prevent repetitions and all unneceſſary prolixity.

Let ABC (*fig.* 1.) be the ſpherical refracting ſurface of ſuch a teleſcope as has been deſcribed, and let the teleſcope be ſuppoſed to be at reſt, or the velocity of light to be infinite with reſpect to that of the earth, and let GBMF be a line drawn from a ſtar at G, in the pole of the ecliptic, through the center M of the refracting ſurface; the image of the ſtar will be formed ſomewhere, as at F, in the line BF; and here the interſection of the croſs wires made uſe of in obſerving muſt be placed. It is evident, that the ſtar will be ſeen in its true direction FG; and we muſt conclude that to be its true direction, becauſe we

know

know that the ray GBF passes into the medium without being refracted by it, and BMF would be considered as the axis of the telescope.

Now let the spherical refracting surface with its wires, or the unusual telescope be carried laterally with the motion of the earth towards Q. Conceive GBF to be a line not partaking of this lateral motion, which at any particular moment passes thro' M, the center of convexity. Along this line suppose one of many rays to pass from a star situated in the pole of the ecliptic. Then will all the contemporary light of this pencil of parallel rays be made to converge so as to meet in a focus somewhere in the unrefracted ray BF. Let F therefore be the point in absolute space where the image of the star is so formed. Let the parallel motion of the telescope, whose refracting spherical surface is ABC, be in the direction of HF, and take FD to FB as the lateral velocity of the telescope to the velocity of light in air, and join BD: then it is manifest, that BD will be the position of a telescope such as Dr. BRADLEY's, when the image of the star is formed in the axis BD, and that IBG, or its equal FBD, will be the angle of greatest aberration.

Moreover, the velocity of the rays as they proceed to the focus F, after refraction at the surface ABC, being supposed the same as in air, it is evident, that the line DML drawn through D, and through the center of convexity M, must give the position of the axis of this kind of telescope, when the image of the star is formed there: for, by hypothesis, the image is formed in F in absolute space, and since BF is supposed to be to FD, as the velocity of light within the medium to the lateral velocity of the telescope, the point D of the axis DL will arrive at F, when the rays arrive there to form

the image. And the obſerver not knowing, or at preſent not taking account of, the lateral motion of the teleſcope, will ſuppoſe, that the line LMD joining the image of the ſtar and the center of convexity M is the true direction of the ſtar; juſt as before he concluded, that FMBG would be the direction of the ſtar when the lateral motion of the teleſcope was ſuppoſed to be nothing. Hence it is evident, that the interſection of the croſs wires, made uſe of in obſerving, muſt now be placed at D; or elſe, if thoſe be ſtill uſed that were before ſuppoſed to be at F, the refracting ſurface ABC with the line or axis BF muſt revolve about the center M till the vertex B comes to L and the croſs wires F to D.

In like manner, if the velocity of the rays were increaſed after refraction at the ſpherical ſurface in any ratio, as that of DF to EF, the refraction continuing the ſame, then EMO drawn through the center of convexity would now give the poſition of the axis of the teleſcope neceſſary for receiving the image formed at F. For the ſpace deſcribed by the rays in paſſing downwards to the focus, in this caſe and the former being equal, the times of their converging at F will be reciprocally as the velocities, or as EF to DF. But, on account of the equable lateral motion of the teleſcope, DF and EF will be as the times of the points D and E arriving at F: therefore, in the laſt caſe, the interſection of the croſs wires ſuppoſed at E will meet the image at F, and accordingly the ſtar will be ſeen in the axis.

Fig. 2. From what has been ſaid it will appear, that if DF be taken to EF, as the ſine of incidence to the ſine of refraction peculiar to the medium which fills the teleſcope; then, from the property of the focus, we ſhall have this proportion, *viz.* BF : FM :: DF : EF. Hence the line EMO paſſing through

through M must be parallel to DB; but DB, as before, denotes the position of Dr. BRADLEY's telescope, when the aberration of the star is at its maximum, and EMO parallel to it, denotes the position of the water telescope, at the same time, upon the supposition that the velocity of the rays without and within be as EF to DF, or inversely, as the sines of incidence and refraction peculiar to water. Here then we discover what must be the law of variation as to the velocity of the rays, provided that the aberration given by such a telescope shall come out the same with that found by Dr. BRADLEY. It is the very same which follows from the Newtonian principles: for from the manner of observing, the angle of aberration is always determined by the position of the telescope necessary for having the image formed somewhere in the axis.

But supposing that in the course of observing with such a telescope, the aberration should come out different from what has already been ascertained by Dr. BRADLEY, it may next be enquired, how from the difference given the velocity of light within the telescope is to be deduced.

Fig. 3. Imagine then such a telescope actually to give FMD as the greatest angle of aberration, and let this be supposed greater than that of Dr. BRADLEY's, which, for example, let be FME. From what has been already said, the velocity of light corresponding to this last mentioned angle, is deducible from the known refraction of the medium which fills the telescope; and, by construction, the velocity corresponding to FMD, the angle given, must be to the former inversely as the tangents of these angles. From this consideration we have the following analogy for finding the velocity corresponding to whatever difference there may be observed between the two aberrations at present alluded to. The rule in all cases must be;

as

"as the tangent of the obſerved angle is to the tangent of the "Bradleyan angle, ſo is the velocity of light deducible from "the hypotheſis of the obſerved angle being the ſame with "that of Dr. BRADLEY to the velocity ſought." It has already been ſhewn, how the former of theſe velocities can be univerſally aſcertained, from the known refraction of the medium which is taken to fill the teleſcope, and therefore the laſt term of the above proportion, which is the velocity ſought, is thereby given.

Fig. 2. In a teleſcope of this kind it will not have eſcaped notice, that the ray BF, which, on account of its paſſing to the focus unrefracted, may be called the axis of the pencil, can never be found in the axis of the teleſcope EO, except at the focus F, where D and F meet. That ray, however, OP, parallel to BC, which falls obliquely on the axis of the teleſcope EO, will continue to paſs along it after refraction, and for that reaſon it may be called the relative axis of the pencil.

This will appear, by conſidering that the particle of light, which at any moment is refracted at the vertex O of the ſpherical ſurface, is found by hypotheſis in the axis a ſecond time, when it meets the cotemporary light at the focus. But ſince both the motion of the axis and of the particle is uniform and rectilinear, the former cannot be found in the latter at two different times, without being found in it continually during the whole interval. In like manner, a part of every other ray from the ſtar, which ſucceſſively falls upon the vertex, muſt move relatively along the axis after refraction: and thus a conſtant ſucceſſion of theſe particles conſtitute a viſual refracted ray, whoſe relative path muſt always be in the axis OE.

All that has been ſhewn concerning the teleſcope already conſidered, will receive ſtill further illuſtration, by tracing the motion

motion of this particular refracted ray till it arrives at the focus. This way of viewing the ſubject will alſo render the reaſoning more general, and make it apply to teleſcopes when the denſe fluid within is ſuppoſed to be confined by object-glaſſes of any figure. But in order to this, it will be convenient to premiſe, and briefly to demonſtrate, what ſhall afterwards be referred to by the name of

PROP. A.

Fig. 4. If any very ſmall body or particle of light as it moves uniformly in the abſolute path SB, has paſſed relatively along a part of the line CD, which advances equably and parallel to itſelf in the direction DK; and if at any inſtant the abſolute path of the particle be changed into any other, as BR; I ſay, it will ſtill paſs relatively along the moving line, provided its velocity now be to its former velocity as the ſine of the angle DBF to the ſine of the angle DBR; theſe being the angles which the moving line BD makes with BF and BR the abſolute path or direction of the particle in the two caſes.

The conſtruction of this figure is ſo ſimple, that it is unneceſſary formally to point it out. Since, by hypotheſis, the velocity of the particle along BR is to its former along BF as the ſine FZ to the ſine RT; or, on account of ſimilar triangles, as DF to IR, and, on account of parallels, as DF to DW, it follows, that the time of its deſcribing BR now, is to the time of formerly deſcribing its equal BF, as DW to DF. But the line BD advancing with a uniform motion, the time of its arriving at W is to the time of its arriving at F, alſo as DW to DF. Therefore, when the particle arrives at R, the point D of the moving line will have arrived at W, and WRP will be its poſition. Hence the particle at that moment muſt be found in the interſection R of this line, with its abſolute path BR. In the

ſame manner it may be ſhewn, that at any other time the particle will be found in the interſection: it, therefore, from the time of its direction being changed at B, muſt paſs relatively along the moving line as before. By a ſmall alteration in the conſtruction it may be ſhewn, that if the abſolute path had been ſo changed at B as to have augmented the angle FBD, ſtill the particle would have moved relatively along DB, provided its velocity after had been to its velocity before as the ſine of FBD the firſt angle to the ſine of the increaſed angle.

To apply, therefore, this propoſition to the preſent inveſtigation, let DB be conceived as the axis of a teleſcope perpendicular to the ſpherical ſurface of a refracting medium which accompanies it in its lateral motion, SB the abſolute path of a particle of light which had paſſed relatively along DB produced, till its arrival at B, and BR its abſolute path within the medium of the teleſcope. Then it is evident, that FBD, or its equal CBS, will be univerſally the angle of incidence, and RBD the angle of refraction. Hence, by prop. A. that ray of the parallel pencil which is refracted at O, the vertex of the ſpherical ſurface in fig. 2. muſt ſtill paſs relatively along the axis, provided the velocity within the teleſcope be to its former in air, as the ſine of incidence to the ſine of refraction. But the image of the ſtar being produced by the meeting of all the contemporary light, will conſequently be found in the axis, which, by hypotheſis, deviates from the true place of the ſtar by the ſame quantity as Dr. BRADLEY's angle; ſo that in this way of conſidering the matter, the ſame thing reſults which was formerly ſhewn in regard to a teleſcope ſo conſtructed.

By prop. A. it is alſo manifeſt, that whatever number of refractions that ray which falls upon the extremity of the axis ſuffers in pervading object-glaſſes of any figure, or even denſe

media

media beyond the object-glaſs if bounded by tranſparent planes to which the axis produced is perpendicular, yet if the velocities and refractions ſo correſpond, ſtill the ray in queſtion will paſs relatively along the axis till it meet the reſt at the focus: for here the refracted ray in the firſt medium becomes the incident ray in relation to its path in the ſecond, and this in its turn becomes an incident ray in relation to its path in the third medium, &c. and therefore by the prop. A. can never deviate from the moving axis whatever be the refractive denſity of the media, or however theſe are diſpoſed in the order of ſucceſſion. And ſince, by Sir ISAAC NEWTON's theorem, the ratio of the ſine of incidence to the ſine of refraction in the paſſage of a ray out of one medium into another, is compounded of the ratio which the former has to the latter in the paſſage of that ray out of the firſt medium into any third, and of the ratio of the former to the latter in the paſſage of the ſame ray out of the third medium into the ſecond, &c. it follows, that if the velocities be related to the degree of refraction as before mentioned, the ray in the laſt denſe medium will, notwithſtanding any number of previous refractions by glaſſes, &c. have the ſame final velocity that would have been acquired on its paſſing immediately out of air into that medium. This being the caſe, it appears, that though the intervention of an object-glaſs may ſhorten the focal diſtance of ſuch a teleſcope, yet it will not diſplace the image nor alter the rule of inferring the final velocity of the rays in the denſe medium from the aberration given; at leaſt when this is ſuppoſed to be the ſame with Dr. BRADLEY's.

Fig. 3. But further, if the aberration of ſuch a teleſcope ſhould differ from the Bradleyan one, and give, for example, the angle OMB, ſtill the ray PO, which falls on O the vertex, muſt be conſidered as an incident ray, which, after refraction,

paſſes along the axis. By prop. A. therefore, the velocity of the ray, whatever this may be after refraction, muſt be to that velocity by which it would have moved relatively in the axis, ſo inclined to its path, previous to the refraction, inverſely as the ſines of incidence and refraction. Now this being duly conſidered, it will be found that the velocity within the medium, correſponding to this ſuppoſed aberration, or the abſolute velocity within the medium, muſt be to the velocity within the medium correſponding to the Bradleyan aberration, inverſely as the tangents of theſe two angles: for let V and v expreſs the velocities before and after refraction correſponding to the Bradleyan angle, and X and x the velocities before and after correſponding to the ſuppoſed uncommon angle, x being the actual velocity after refraction; then, becauſe by prop. A. the antecedent is to the conſequent, in both caſes, in the ſame ratio, *viz.* as the ſine of refraction to the ſine of incidence, it will be $V : v :: X : x$, and therefore $V : X :: v : x$. But from the nature of the aberration V muſt be to X (this ſuppoſititious velocity before incidence) inverſely as the tangents of the angles of the two aberrations. This therefore muſt be the ratio of v to x. But v is given as before ſhewn; therefore x the velocity within the medium correſponding to the ſuppoſed obſerved aberration is alſo given, and by the ſame rule as was found formerly in the caſe of the firſt teleſcope.

What has been at preſent advanced is unconnected with any hypothetical notions concerning the rays or the cauſe of refraction. Light has been conſidered only as ſomething which moves uniformly from one place to another, and which is always refracted according to a known law. The firſt of theſe properties has been put beyond all doubt by the obſervations of Dr. BRADLEY and Mr. MOLYNEUX; and it is has been long known that the laſt is quite agreeable to experience.

It has indeed always been taken for granted, that the velocity of the ray which paſſes through the center of convexity, repreſents the common velocity of all the contemporary light of the converging pencil. This may perhaps be reckoned a circumſtance of which we have no proof. But it muſt be conſidered, that if the rays of light, after being variouſly bent towards the focus, were no longer to move with the ſame common velocity, the image formed at the focus of Dr. BRADLEY's teleſcope, would be elongated in the direction of the aberration. Thoſe who have attended to this ſubject will be at no loſs in diſcerning the reaſon of this. The extent of that lengthened image would depend upon the difference of velocity which would obtain among the converging rays, and would probably increaſe according to the largeneſs of the aperture of the object-glaſs. But ſuch a phenomenon being contrary to experience, it follows, that the unequal bending of the rays does not give them unequal velocities, whilſt moving in the ſame medium. This is another property with regard to the motion of light which may be conſidered as proved experimentally by Dr. BRADLEY's obſervations, and which doubtleſs would have occurred to him if he had had occaſion to trace the refraction of a pencil of parallel rays at the object-glaſs of his teleſcope.

To conclude: in bringing this queſtion concerning the velocity of light to the iſſue of an experiment, that fluid would doubtleſs be moſt proper for the teleſcope which abſorbs the feweſt rays, and poſſeſſes the greateſt refractive denſity, and which at the ſame time is not liable to generate air-bubbles. To compenſate for the unavoidable loſs of light, which by Mr. CANTON and Dr. PRIESTLEY's experiments is found to be conſiderable in ſuch caſes, it perhaps may be neceſſary to uſe an achromatic

achromatic object-glaſs for the ſake of a large aperture, and of ſuch a figure as to ſhorten the focal diſtance as much as the obſervations of ſuch a ſmall angle can admit of. Some contrivance too will be requiſite to keep the whole ſpace between the object-glaſs and the eye-glaſs always full, notwithſtanding the expanſions and contractions of the confined fluid by heat and cold, or its waſte by evaporation.

It might prove a very conſiderable abridgement of the neceſſary apparatus, if this kind of teleſcope could be connected with the common teleſcope of a mural quadrant, or zenith ſector, and their axes made perfectly parallel by previous obſervations of a proper terreſtrial object. But as there would be ſome room for apprehending that the exact adjuſtment of the axes might be affected in raiſing the teleſcopes afterwards for celeſtial obſervations, this might be examined into by directing them to ſome ſtar ſituated in, or very near, the ecliptic, and taking its meridian altitudes at a time of the year when it is in quadrature with the ſun, in which caſe it would have no aberration. But either in this way, or with two ſeparate inſtruments, the experiment might be made in a few nights, by taking the zenith diſtance of a proper ſtar, the plane of the inſtruments being alternately turned different ways in obſerving, to get the true zenith diſtance independent of the error of the line of collimation; or the meridian altitude of the pole ſtar may be obſerved in December above and below the pole, which will give the apparent diſtance of the ſtar from the pole at that time as affected by aberration. The error of the line of collimation would not affect the reſult in this way, being the ſame in the obſervation both above and below the pole.

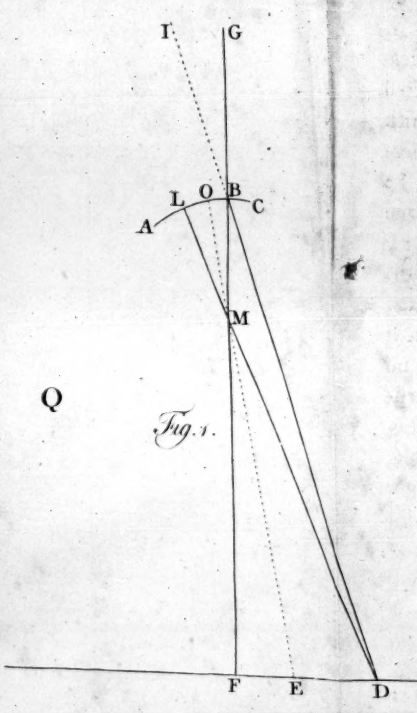

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Fig. 1.

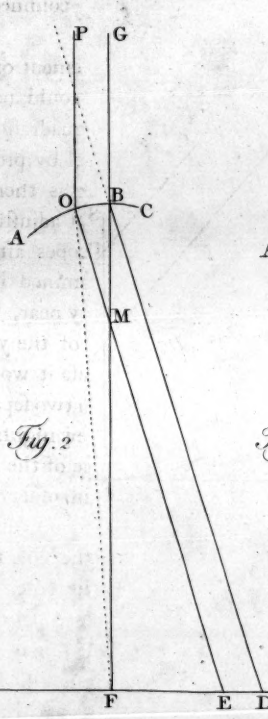


Fig. 2

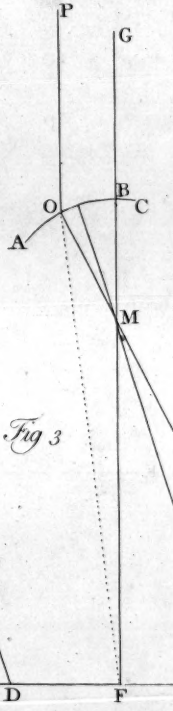


Fig 3

G

B
C

M

F E D

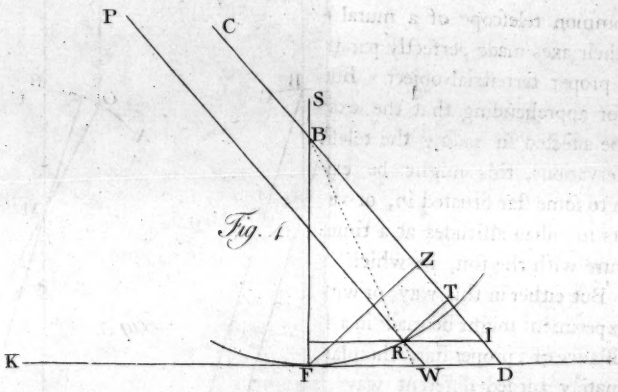


Fig. 1

Basire f.

IX. *Quantity of Rain which fell at* Barrowby *near* Leeds *. By George Lloyd, Esq. F. R. S.*

Read January 31, 1782.

	1778	1779	1780	1781	Averages.
	Inches.	Inches.	Inches.	Inches.	Inches.
January	2.1	0.6	0.9	1.2	1.2
February	1.3	0.0	1.4	2.7	1.35
March	2.1	0.4	0.5	0.1	0.775
April	0.3	2.0	4.3	1.5	2.025
May	1.8	3.4	1.4	1.1	1.925
June	2.1	2.15	0.8	3.2	2.0625
July	3.9	5.6	1.3	1.6	3.1
Auguſt	0.33	2.25	2.45	3.4	2.1075
September	2.87	3.55	4.55	5.6	4.1425
October	5.3	2.8	3.15	0.3	2.8875
November	2.6	2.1	1.95	2.8	2.3625
December	3.3	4.2	0.2	2.1	2.45
Totals	28.0	29.05	22.9	25.6	26.3875

* See Phil. Tranſ. vol. LXVII. part I. p. 512.

X. *Account of an improved Thermometer. By Mr.* James Six; *comunicated by the Rev. Mr.* Wollaſton, *F. R. S.*

Read February 28, 1782.

ATTEMPTING ſome time ago to aſcertain the greateſt degree of heat and cold that happened in the atmoſphere each day and night, or during the courſe of twenty-four hours, I experienced the inconvenience which attends thermometers commonly made uſe of for that purpoſe; the neceſſity I mean of the obſerver's eye being on the inſtrument the very inſtant the mercury ſtands at the higheſt or loweſt degree: for, ſince the time when that may happen is utterly uncertain, if it be not immediately noticed, it can never after be known. The ſultry heat of the ſummer's days, and freezing cold of the winter's nights, which is commonly moſt ſevere at a late unſeaſonable hour, render it very unpleaſant to be abroad in the open air, although it is abſolutely neceſſary for the thermometer to be placed in ſuch a ſituation. Ingenious men of our own country, as well as foreigners, have, it ſeems, long ago endeavoured to remedy this inconvenience; and ſeveral thermometers of different conſtructions have been invented for that purpoſe. VAN SWINDEN deſcribes one which he ſays was the firſt of the kind, made on a plan communicated by Mr. BERNOULLI to Mr. LEIBNITZ. Mr. KRAFT, he alſo tells us, made one nearly like it *. A deſcription of thoſe by Lord

* Diſſ. ſur la Comparaiſon du Therm. par VAN SWINDEN, p. 253–255.

CHARLES

CHARLES CAVENDISH and Mr. FITZGERALD may be seen in the Philosophical Transactions *. Though much ingenuity appears in the invention of those curious instruments, I could not forbear thinking, that a thermometer might be constructed more conveniently to answer the purpose, and shew accurately the greatest degree of heat and cold which happened in the observer's absence. I therefore attempted to make one: with what success I submit to your better judgement, and proceed to give a description of the instrument. Fig. 1. *a b* is a tube of thin glass, about sixteen inches long, and five sixteenths of an inch in diameter; *c d e f g h* a smaller tube with the inner diameter, about one fortieth, joined to the larger at the upper end *b*, and bent down, first on the left side, and then, after descending two inches below *a b*, upwards again on the right, in the several directions *c d e*, *f g h*, parallel to, and one inch distant from it. On the end of the same tube at *h*, the inner diameter is enlarged to half an inch from *h* to *i*, which is two inches in length. This glass is filled with highly rectified spirits of wine to within half an inch of the end *i*, excepting that part of the small tube from *d* to *g*, which is filled with mercury. From a view of the instrument in this state, it will readily be conceived, that when the spirit in the large tube, which is the bulb of the thermometer, is expanded by heat, the mercury in the small tube on the left side will be pressed down, and consequently cause that on the right side to rise; on the contrary, when the spirit is condensed by cold, the reverse will happen, the mercury on the left side will rise as that on the right side descends. The scale, therefore, which is FAHRENHEIT's, beginning with *o* at the top of the left side, has the degrees numbered downwards, while that at the right

* Phil. Trans. vol. L. p. 501. and vol. LI. p. 820.

ſide, beginning with *o* at the bottom, aſcends. The diviſions are aſcertained by placing this thermometer with a good ſtandard mercurial one in water gradually heating or cooling, and marking the diviſions of the new ſcale at every 5°*. Thus far our thermometer reſembles in ſome reſpects thoſe of Mr. BERNOULLI and Lord CHARLES CAVENDISH; but the method of ſhewing how high the mercury had riſen in the obſerver's abſence, the eſſential property of an inſtrument of this kind, is wholly different from theirs, and effected in the following manner. Within the ſmall tube of the thermometer, above the ſurface of the mercury on either ſide, immerſed in the ſpirit of wine, is placed a ſmall index, ſo fitted as to paſs up and down as occaſion may require: that ſurface of the mercury which riſes carries up the index with it, which index does not return with the mercury when it deſcends; but, by remaining fixed, ſhews diſtinctly, and very accurately, how high the mercury had riſen, and conſequently what degree of heat or cold had happened. Fig. 2. repreſents one of theſe indexes drawn larger than the real ones, to render it more diſtinct. *a* is a ſmall glaſs tube, three quarters of an inch long, hermetically ſealed at each end, incloſing a piece of ſteel wire, nearly of the ſame length; at each end *c d* is fixed a ſhort piece of a tube of black glaſs, of ſuch a diameter as to paſs freely up and down within the ſmall tube of the thermometer. The lower end, floating on the ſurface of the mercury, is carried up with it when it riſes, while the piece at the upper end, being of the ſame diameter, keeps the body of the index parallel to the ſides of the thermometrical tube. From the upper end of the body of the index at *c* is drawn a ſpring of glaſs to

* The diviſions below the freezing point are taken by means of a mixture of ſea ſalt and ice, as deſcribed by NOLLET, DE LUC, and others.

the

the fineneſs of a hair, about five ſevenths of an inch in length, which, being ſet a little oblique, preſſes lightly againſt the inner ſurface of the tube, and prevents the index from following the mercury when it deſcends, or being moved by the ſpirit paſſing up or down, or by any ſudden motion given to the inſtrument by the hand or otherwiſe; but at the ſame time the preſſure is ſo adjuſted as to permit this index to be readily carried up by the ſurface of the riſing mercury, and downwards whenever the inſtrument is be rectified for obſervation. To prevent the ſpirit from evaporating, the tube at the end *i* is cloſely ſealed *. Fig. 3. repreſents the thermometer on its frame; the plates on which the ſcale is graved on either ſide are made to ſlide out, and the frame is open to the back, behind the large tube, which does not touch it, except at each end. The cap *a*, and the baſe *b*, are made to fix on with ſcrews, and only cover the turning of the ſmall tube. By a ſcrew at the bottom of the frame, it may be made faſt to the wall againſt which it is to hang without doors, to prevent its being ſhaken by violent winds. Towards evening I uſually viſit my thermometer, and ſee at one view, by the index on the left ſide, the cold of the preceding night; and by that on the right, the heat of the day. Theſe I minute down, and then apply a ſmall magnet to that part of the tube againſt which the indexes reſt, and move each of them down to the ſurface of the mercury: thus, without heating, cooling, ſeparating, or at all diſturbing the mercury, or moving the inſtrument, may this

* When this tube is cloſed (not hermetically, but only ſo as to prevent the ſpirits evaporating) the thermometer muſt be brought to the greateſt heat it is likely at any time after to ſuſtain; and though no more air is incloſed than what remains at that time above the ſpirits, yet that will, by its elaſticity preſſing on the fluid, anſwer every purpoſe as well as if the external air was freely admitted.

thermometer, without a touch, be immediately rectified for another obſervation. When I wiſh to put the thermometer out of my hand, without hanging it up, I have a ſtand to place it on; for if the mercury preſſes againſt the index, while the inſtrument lies in an horizontal poſition, it is in danger of paſſing by it, which is avoided by keeping the thermometer in a poſition nearly vertical. To prevent the mercury ſhifting its place in the ſpirits within the tube (which I apprehended it might do on account of the ſuperiority of its ſpecific gravity, eſpecially when kept for a conſiderable time, very high on one ſide, and low on the other), I made that part of the ſmall tube from *e* to *f* with the inner diameter exceeding ſmall; and found, upon trial, that after the ſummer's heat had kept the mercury for a long time high on one ſide, the winter's cold brought it again as accurately to the freezing point on the other as at firſt *. This thermometer may be made a mercurial one by inverting the glaſs, and filling with mercury that part which in the firſt is filled with ſpirits, and with ſpirits that part of the ſmall tube from *d* to *g* which in the former is filled with mercury; the indexes in either caſe may be the ſame, and will be carried up in the ſame manner upon the ſurface of the mercury; but the end of the tube at *i*, inſtead of being ſealed, muſt then be left open, and ſtand inverted in a bulb, or ſmall ciſtern of mercury, into which the external air has free acceſs. The diameter of the tube *a b* ſhould be conſiderably increaſed if the degrees on the ſcale are required to be as wide as thoſe in the ſpirit thermometers. It is indeed better in this caſe to have a double rather than a larger ſingle tube; but finding the weight of ſo great a quantity of mercury in a thin glaſs tube

* With a thermometer of this ſort I obſerved the greateſt heat and cold that happened every day and night throughout the year 1781.

attended with many diſadvantages, and the motion of the fluids in the ſpirit-ones perfectly agreeing with, and being as readily excited by change of heat and cold, as in the mercurial thermometers, I preferred the former as much more commodious. A perſon cannot approach near to the thermometer firſt deſcribed when the air is very cold (eſpecially with a light which by night is neceſſary) without cauſing the ſpirits preſently to expand, and conſequently the mercury on the left ſide immediately to deſcend. This ſenſibility is here attended with every advantage, without the inconvenience to which common thermometers in this caſe are liable *; for the index will accurately ſhew the greateſt height to which the mercury had riſen, although, before the exact degree can well be diſtinguiſhed, it will appear ſeparated from the index, and deſcending apace. As the ſcale is ſixteen inches long, and divided into 100° only, which are more than ſufficient for the temperature of the air, they are large enough to be ſub-divided at pleaſure. The indexes, though of a tender and delicate nature, when once placed in the tube, are not liable to ſuffer any alteration by time or accident; and the thermometer may be expoſed to rain at all times, without ſuffering the leaſt injury in any reſpect.

In conſtructing the thermometer before mentioned, I at firſt hit on a plan by which the ſame end was obtained by a dif-

* The moſt ſenſible mercurial thermometers commonly have the column of mercury as well as the degrees very ſmall, and a perſon aſſiſted with a light can hardly view them near enough, when the weather is very cold, without cauſing the mercury to riſe before the degrees where it ſtood can be well aſcertained.

Freezing fogs alſo, which with us uſually attend the greateſt degrees of cold, by covering the glaſs with froſt, render the mercury inviſible, and cannot well be mercury removed without cauſing the to riſe, or at leaſt render the obſervation doubtful, which at ſuch a time is very diſagreeable; for, in proportion to the extraordinary degree of cold, ſo is our curioſity likely to be excited.

ferent

ferent method; and though, in some respects, and for some purposes, it may not be so proper as that already described, yet, for some others, it may be found useful, and therefore I shall briefly describe it. The glass of this instrument is in all respects the same as in the former, excepting that the diameters of the tubes are something larger. It is likewise filled with spirits of wine and mercury, in the same manner; but the indexes are different, being only a small tube of black glass, about five-sevenths of an inch in length, hermetically sealed at each end, containing a piece of steel wire. An index of this sort is placed in the thermometer on either side, which, having no spring to support them, sink down in the spirits, and rest upon the mercury. Whenever the mercury descends, the index will follow it; but when it rises, the index will not rise with it, and by remaining at the place to which the mercury had descended, will shew the greatest degree of heat or cold which had happened. In this manner do these indexes answer the same purpose, though they move directly contrary to the others in the other thermometer; but this instrument is not so easily rectified as the former, for the most powerful magnet will not bring the index up again while the mercury above presses against them; and although it is possible to remove the mercury, and by that means set the index at liberty, yet inconveniences will be incurred from which the other is entirely free.

In some cases it may be found expedient, instead of the double thermometer first described, to make two single ones; one to shew the greatest degree of heat only, and the other the cold, each having its proper index (see fig. 4. and 5.). The first has the small tube bent down on the left side, and the lower end immersed in a bulb or small cistern of mercury, to which the external air has free access; the other has the small

tube

tube turned up on the right ſide, with ſome mercury let down to the bottom, and the upper end cloſely ſealed, as in the double inſtrument. Making a ſtandard mercurial thermometer, by which the ſcale of the ſpirit one was to be divided, I endeavoured to obtain as wide degrees as poſſible, that the motion of the mercury might thereby be rendered more conſpicuous, and the height of it aſcertained with greater preciſion. It is true, the larger the degrees, the larger in ſome meaſure muſt be the bulb, and therefore the fluid contained in it not likely to be ſo ſoon affected by any change of heat or cold in the atmoſphere as in a ſmaller. But as this thermometer was principally to be uſed immerſed in a large quantity of water, gradually heating or cooling, little or no diſadvantage could ariſe from making the bulb ſomewhat larger than thoſe commonly made uſe of in the air. Not being able, however, to procure glaſs tubes ſo long as I had occaſion for, whoſe inner diameters were perfectly equal, I took the following method to adjuſt the diviſions on the ſcale to the inequality of the tubes. Chooſing a tube of a length ſuitable to my purpoſe, with a proper bulb at the end, I put into it a ſmall quantity of mercury * ſufficient to form a column about one inch in length. Drawing then on a board the three lines *aa*, *bb*, *cc*, fig. 6. I placed the glaſs tube on the line *aa*, and while the mercury remained at reſt at the end of the tube, near the bulb, I made two pencil marks on the line *aa*, one at *d*, and the other at *e*, perfectly coin-

* To put in a ſmall quantity of mercury, and meaſure its length at different parts of the tube, as deſcribed by Abbé NOLLET, vol. IV. p. 370. Lecons Phyſique, is a very excellent method to diſcover the error; but in what manner readily to adjuſt the ſcale, ſo as to avoid any inaccuracy from ſuch inequality (which in tubes of the length I had occaſion for ſeemed to me unavoidable) was a matter concerning which I could meet with no information.

ciding

ciding with the two ends of the column of mercury: then causing the mercury to move slowly on farther from the bulb, till that end of the column which was first at *d* coincided with the mark at *e*, and letting it rest again, I made another mark at *f*; after which, causing the mercury to move on as before, and continuing to mark its length at every part of the tube till it reached the end farthest from the bulb; by these means I obtained the several intermediate points on the line *aa*. Through these several points I drew dotted lines parallel to each other, and at right angles with the line *aa* to the line *bb*. Taking now, with a pair of compasses, the widest intervals between any of the dotted parallels, which in this case is from *d* to *e*, I inserted that distance successively between the several parallels, beginning at the lowest pair, as from *d* to *e*, from *e* to *f*, from *f* to *g*, and so on to *h*, as exhibited in the figure; and the aggregate of these lines may be considered as one continued line, without any error of consequence in this matter. Having now the thermometer completely filled with mercury, the air expelled, the point of the scale at 102°, and the freezing point properly taken * and marked upon the tube, which was now hermetically sealed, I again applied the tube to the line *aa*, and marked on that line the point of 102° and the freezing point. Through those points I drew the lines *ii*, *kk*, and divided that part of the compound line *dh* included between *ii*

* The freezing point, marked on the tube of this thermometer, is immediately taken by means of grated ice; but the point of 100° by a standard mercurial thermometer, the upper point of the scale of which was properly taken by boiling water, and the lower one by grated ice; but it is more commodious in the first to have the tube no longer than the air scale, especially as the degrees are pretty wide. The method of adjusting the scale to the inequality of the tube remains the same, let the given points be at any distance, or the divisions increased to any number.

and

Fig. 1

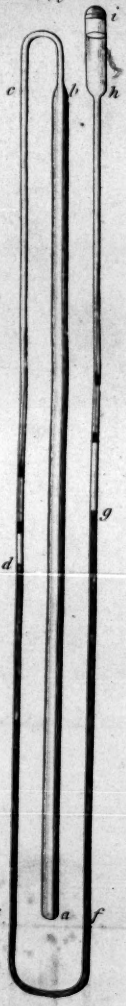


Fig. 2

Fig. 3

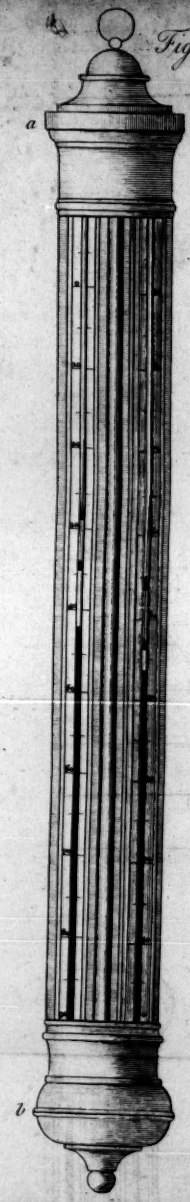


Fig. 4

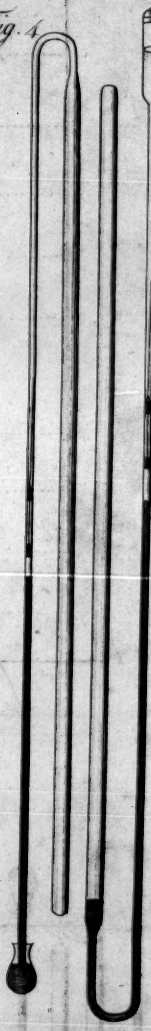
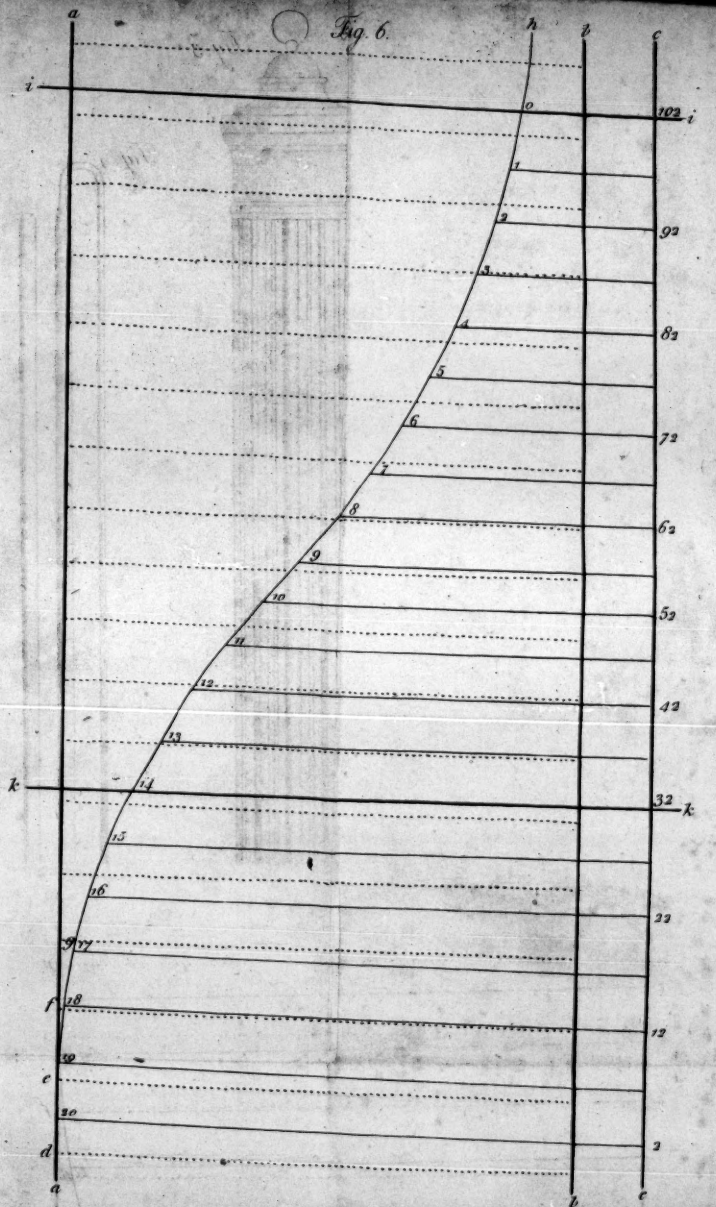


Fig. 5

Fig. 6.


a
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and *kk* into 14 equal parts, beginning at *o*, the point where *ii* cuts the line *dh*, continuing afterward ſix diviſions now on that line below *kk*, making in all 20 equal diviſions. If now lines be drawn through each of the dividing points, from *o* to 20 to the line *cc*, at right angles with the ſame, they will give on the line *cc* the true thermometrical ſcale to every 5° from 2 to 102, properly adjuſted to the inequality of the tube *, which in this caſe is nearly of the ſame diameter at each end, but ſmaller towards the middle. Tubes may indeed be found of ſome conſiderable length with leſs inequality than what this ſcale exhibits; but the error is here enlarged, to render the method of correcting it more conſpicuous.

* Experimentally to prove this method I have made mercurial thermometers, whoſe ſcales from the freezing point to that of boiling heat were nearly three feet, and though the inequalities of the tubes were very conſiderable, varying in contrary directions to each other; yet when they were placed on the ſame frame, they perfectly agreed in a motion of the mercury in every part of their ſcales.

XI. *On the Parallax of the Fixed Stars.* By *Mr.* Herſchel, *F. R. S.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read December 6, 1781.

TO find the diſtance of the fixed ſtars has been a problem which many eminent aſtronomers have attempted to ſolve; but about which, after all, we remain in a great meaſure ſtill in the dark. Various methods have been purſued without ſucceſs, and the reſult of the fineſt obſervations has hardly given us more than a diſtant approximation, from which we may conclude, that the neareſt of the fixed ſtars cannot be leſs than forty thouſand diameters of the whole annual orbit of the earth diſtant from us. Trigonometry, by whoſe powerful aſſiſtance the mathematician has boldly aſcended into the planetary regions, and meaſured the diameters and orbits of the heavenly bodies, for want of a proper baſe, can here be but of little ſervice; for the whole diameter of the annual orbit of the earth is a mere point when compared to the immenſe diſtance of the ſtars. Now, as it is not in our power to enlarge this baſe, we can only endeavour to improve the inſtruments by which we meaſure its parallax.

There are two things requiſite for meaſuring extremely ſmall angles with accuracy. Firſt, that the inſtrument we uſe for this purpoſe, be it quadrant, ſector, or micrometer, ſhould be divided and executed with ſufficient exactneſs; and, ſecondly, that the teleſcope, by which the obſervations are to be made,

made, ſhould have an adequate power and diſtinctneſs. Upon the firſt head, the great improvements of mathematical inſtrument-makers have hardly left us any thing to deſire: we can now meaſure ſeconds with almoſt as much facility and truth as former obſervers could meaſure minutes; nor do I think it impoſſible to go ſtill further, and divide inſtruments that would ſhew thirds with ſufficient accuracy. It is in the latter, or optical part, we find the greateſt difficulty. To ſee a ſingle ſecond of a degree with preciſion requires a teleſcope of very great perfection; therefore, ſuppoſing the mechanical part of an apparatus well executed, it will ſtill be neceſſary to try how far the power of our teleſcope will enable us to aſcertain with confidence the diviſion or number of ſeconds it points out. If upon trial we find that our inſtrument will give us the ſame meaſure within the ſecond, every time the experiment is repeated, we may pronounce it capable of meaſuring ſeconds; if otherwiſe, it will remain to be examined, whether the fault lies in the mechanical or optical part.

Let us now ſuppoſe that the parallax of the fixed ſtars does not amount to a ſingle ſecond, yet ſtill the caſe is by no means deſperate; and though the difficulty of meaſuring ſeconds will ſoon ſuggeſt to us what extraordinary powers and diſtinctneſs of the teleſcope, and accuracy of the micrometer, are required to meaſure thirds; this ought by no means to diſcourage us in the attempt. Could we meaſure angles, much ſmaller than ſeconds, might we not hope to find the parallax of ſome of the fixed ſtars at leaſt to amount to ſeveral thirds? On the other hand, if it ſhould appear, indeed, that even with ſuch improved methods of meaſurement we could not reach the remote ſituation of ſuch almoſt infinitely diſtant ſuns, we might ſtill derive a valuable approximation towards truth from

ſuch repeated obſervations, even though they ſhould not be attended with all the ſucceſs we expected from them. On this aſſurance, I endeavoured to take ſuch a method for attempting the inveſtigation of the parallax of the ſtars as to avail myſelf of the improvements I had already made, and was ſtill in hopes of making, in my teleſcopes.

The next thing that was neceſſary to conſider in this undertaking was, the manner of putting it into execution. The method pointed out by GALILEO, and firſt attempted by HOOK, FLAMSTEAD, MOLINEUX, and BRADLEY, of taking diſtances of ſtars from the zenith that paſs very near it, though it failed with regard to parallax, has been productive of the moſt noble diſcoveries of another nature. At the ſame time it has given us a much juſter idea of the immenſe diſtance of the ſtars, and furniſhed us with an approximation to the knowledge of their parallax that is much nearer the truth than we ever had before. Dr. BRADLEY, in a letter to Dr. HALLEY on the ſubject of a new diſcovered motion of the fixed ſtars, ſays, "I believe I may venture to ſay, that in either of the "two ſtars laſt mentioned (γ Draconis and η Urſæ majoris) it "(the annual parallax) does not amount to $2''$. I am of opi-"nion, that if it were $1''$ I ſhould have perceived it in the "great number of obſervations that I made, eſpecially upon "γ Draconis; which agreeing with the hypotheſis (without "allowing any thing for parallax) nearly as well when the "ſun was in conjunction with, as in oppoſition to, this ſtar, "it ſeems very probable, that the parallax of it is not ſo great "as one ſingle ſecond." Phil. Tranſ. n. 406. p. 637. Dec. 1728. As I do not know that any thing more deciſive has been done upon the ſubject, it will not be amiſs to ſee how far this method of finding the parallax has really been ſucceſsful.

The

The inſtrument that was uſed upon this occaſion was the ſame as the preſent zenith ſectors, which can hardly be allowed ſufficient to ſhew an angle of one or even two ſeconds with accuracy; yet, on account of the great number of obſervations, and above all the great ſagacity of the obſerver, we will admit that if the parallax had amounted to two ſeconds he would have perceived it. The ſtar on which theſe obſervations were made is marked of the third magnitude in the catalogue of PTOLEMY; in TYCHO BRAHE's of the third; in the Prince of HESSE's of the third; in HEVELIUS's between the third and ſecond; in FLAMSTEAD's of the ſecond; and now appears as a very bright ſtar of the third, or ſmall ſtar of the ſecond magnitude; therefore its parallax is probably conſiderably leſs than that of a ſtar of the firſt magnitude. Several authors who have touched upon this ſubject ſeem to have overlooked this diſtinction; and from Dr. BRADLEY's account of the parallax of γ Draconis, have concluded the parallax of the ſtars in general not to exceed $1''$; but this appears to me by no means to follow from the doctor's obſervations. It is rather evident that, for aught we know to the contrary, the ſtars of the firſt magnitude may ſtill have a parallax of ſeveral ſeconds; and I believe this to be as accurate a reſult as that method is capable of giving, at leaſt in latitudes where there is not a ſtar of the firſt magnitude that paſſes directly through the zenith *.

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*' DE LA LANDE, in his excellent book of Aſtronomy, ſays, that the parallax of the fixed ſtars has been proved to be abſolutely inſenſible (Aſt. liv. XVI. § 2782.). He reports the obſervations of TYCHO BRAHE, PICARD, HOOK, and FLAMSTEAD, and concludes (§ 2778.) from the diſcovery of the aberration by Dr. BRADLEY (which it ſeems he alſo allows to be the moſt deciſive upon the ſubject) that now the queſtion about parallax is reſolved. In giving us the opinion which the

In general, the method of zenith diſtances labours under the following conſiderable difficulties. In the firſt place, all theſe diſtances, though they ſhould not exceed a few degrees, are liable to refractions; and I hope to be pardoned when I ſay that the real quantities of theſe refractions, and their differences,

the doctor had of the reſult of his own obſervations with regard to the annual parallax, DE LA LANDE only mentions "M. BRADLEY penſe que ſi elle (la "parallaxe) eût été ſeulement de 1″ il l'auroit apperçue dans le grand "nombre d'obſervations qu'il avoit faites, ſurtout de γ du Dragon." But if we alſo take in thoſe lines upon which Dr. BRADLEY ſeems to lay the greateſt ſtreſs, viz. "I believe I may venture to ſay, that in either of the "two ſtars laſt mentioned it does not amount to two ſeconds;" and if we allow for the magnitude of the ſtars upon which the obſervations were made, I think I have fairly ſtated the full amount of all the actual proofs we have of the ſmallneſs of the annual parallax. Now, ſince it has eſcaped the fineſt obſervations of BRADLEY, it is not likely that it ſhould come up to the full quantity to which it might amount without being perceived; and therefore the doctor might think it highly probable, "that it is not ſo great as one ſingle ſecond;" and his opinion, as well as DE LA LANDE's, who believes it to be abſolutely inſenſible, are perfectly conſiſtent with all the obſervations that have hitherto been made; though the *actual proofs*, which are the ſubject of our preſent inquiry, do not extend ſo far. Againſt the parallax of Sirius DE LA LANDE (§ 2781.) mentions "forty-"five meridian altitudes taken by Dr. BEVIS [*a*], with the eight-feet mural qua-"drant of the Royal Obſervatory at Greenwich, none of which differed 3 or 4″ "from the mean altitude." Now, if they differed 3 or 4″ from the mean, we may ſuppoſe they differed 6 or 8″ from each other; and that obſervations, ſubject to ſo many cauſes of error as I ſhall preſently enumerate, and which differed ſo much from each other, cannot give the leaſt evidence either for or againſt a parallax, will need no proof. Refraction alone, which is liable to ſuch changes at the meridian altitude of Sirius, notwithſtanding the moſt careful obſervations of the barometer and thermometer ſhould be made to aſcertain its quantity, would, with me, remain an unanſwerable argument againſt the validity of ſuch obſervations in a ſubject of this critical nicety.

[*a*] Theſe obſervations were not made by Dr. BEVIS, but extracted from the regiſters of the Royal Obſervatory at my deſire, and calculated by myſelf, and ſent in a letter by Dr. BEVIS to Paris.

NEVIL MASKELYNE.

are very far from being perfectly known. Secondly, the change of position of the earth's axis arising from nutation, precession of the equinoxes, and other causes, is so far from being completely settled, that it would not be very easy to say what it exactly is at any given time. In the third place, the aberration of light, though best known of all, may also be liable to some small errors, since the observations from which it was deduced laboured under all the foregoing difficulties. I do not mean to say, that our theories of all these causes of error are defective; on the contrary, I grant that we are for most astronomical purposes sufficiently furnished with excellent tables to correct our observations from the above mentioned errors. But when we are upon so delicate a point as the parallax of the stars; when we are investigating angles that may, perhaps, not amount to a single second, we must endeavour to keep clear of every possibility of being involved in uncertainties; even the hundredth part of a second becomes a quantity to be taken into consideration.

I shall now deliver the method I have taken, and shew that it is free from every error to which the former is liable, and is still capable of every improvement the telescope and mechanism of micrometers can furnish.

Let OE (fig. 1.) be two opposite points of the annual orbit, taken in the same plane with two stars *a*, *b*, of unequal magnitudes. Let the angle *a*O*b* be observed when the earth is at O: and let the angle *a*E*b* be also observed when the earth is at E. From the difference of these angles, if any should be found, we may calculate the parallax of the stars, according to a theory that will be delivered hereafter. These two stars, for reasons that will soon appear, ought to be as near each other as possible,

poſſible, and alſo to differ as much in magnitude as we can find them.

GALILEO, I believe, was the firſt who ſuggeſted this method; but in the manner he mentions it in his third dialogue of the *Syſtema Coſmicum*, it would be expoſed to all the difficulties we have enumerated, and would wiſh to avoid; for he does not obſerve, that the two ſtars ſhould be ſo near each other as thereby to preclude the influence of every cauſe of error.

This method has alſo been mentioned by other authors; and we find that Dr. LONG obſerved the double ſtar which is the firſt of Aries in PTOLEMY's catalogue; that in the head of Caſtor; the middle one in the ſword of Orion; and that in the breaſt of Virgo, with teleſcopes of fourteen and ſeventeen feet, and "was perſuaded they would be found always to appear "the ſame." But when the theory of parallax will be explained, it will be ſeen that every one of theſe ſtars are totally improper for the purpoſe; for the ſtars of γ Arietis are near 10″ diſtant from each other, and moreover equal in magnitude. In α Geminorum the ſtars, though near enough, do not ſufficiently differ in magnitude to ſhew any parallax. The ſtars in the Nebula of Orion, on account of their extreme ſmallneſs or diſtance, are ſtill more improper than any; and thoſe of γ Virginis are equal in magnitude.

I do not find that any thing elſe has been done upon the ſubject. GALILEO juſtly remarks, that ſuch obſervations ought to be made with the beſt teleſcopes, and upon this occaſion mentions the power of his own, which enlarged the diſk of the ſun a thouſand times, from which we find it magnified about thirty-two times; but we can hardly think his nor even Dr. LONG's, whoſe power might probably be ſixty or ſeventy, ſufficient for the purpoſe. What would GALILEO ſay, if he were told that

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our preſent opticians make inſtruments that enlarge the diſk of the ſun above forty thouſand times? What would even CASSINI ſay, if he were to view the firſt ſtar of Aries, which appeared to him as ſplit in two, through a teleſcope that will ſhew γ Coronæ borealis and *b* Draconis to be double ſtars?

But to proceed, I ſhall now prove that this method, if ſtars properly ſituated (ſuch as I have found) are taken, is free from all the errors occaſioned by refraction, nutation, preceſſion of the equinoxes, changes of the obliquity of the ecliptic, and aberration of light; and that the annual parallax, if it even ſhould not exceed the tenth part of a ſecond, may ſtill become viſible, and be aſcertained at leaſt to a much greater degree of approximation than it ever has been done.

It will alſo appear, from the great number of obſervations I have already made upon ſeveral double ſtars, eſpecially ε Bootis, that we can now with much greater certainty affirm the annual parallax to be exceedingly ſmall indeed; and that there is a great probability of ſucceeding ſtill farther in this laborious but delightful reſearch, ſo as to be able at laſt to ſay, not only how much the annual parallax *is not*, but how much it really *is*.

Let there be two ſtars at a diſtance from each other, not exceeding five ſeconds; ſuppoſe them to be obſerved at an altitude of 20°; and let them be ſo ſituated with reſpect to each other, that one of them may be 20°, and the other 20° and 5″ high: then the whole effect of mean refraction at that altitude, by Dr. MASKELYNE's excellent tables, will be 2′ 35″,5 for 20°, and 2′ 35″,4888 for 20° 5″. The difference is 0″,0111. Now, in the firſt place, we have nothing to do with the refraction itſelf, ſince the real altitude of the ſtars is not in queſtion. In the next place, we alſo have no concern with the difference of refrac-

tion between the two ſtars, though no more than the ,0111th part of a ſecond, becauſe the real diſtance between the two ſtars is not required. It follows then, that theſe obſervations can only be affected by the difference of the difference; that is, by an alteration in the quantity of refraction occaſioned by the change of heat and cold, or weight of the atmoſphere, and pointed out to us by the riſe and fall of the barometer and thermometer. Let us then ſee what this difference of the difference may amount to. Suppoſe a change of 22° of FAHRENHEIT's thermometer, that is, from the freezing point to the moderate air of a ſummer's night, and a difference of an inch in the height of the barometer; theſe two cauſes both conſpiring, which does not often happen, may occaſion an alteration of ,0096th part of a ſecond in five, at an altitude of 20°; but this being leſs than the thouſandth part of a ſecond may ſafely be rejected as a quantity altogether inſenſible.

Since it may not be always convenient to view theſe ſtars at the altitude of 20°, it remains to ſee what effect different altitudes may have: let us then make the moſt unfavourable ſuppoſition, that they may one time be ſeen in a horizontal poſition, having before been ſeen vertical. In this caſe, as the whole difference of refraction in a difference of 5″ of altitude is no more than ,0111, provided they are obſerved not lower than 20°, and the whole difference of the difference of refraction is only ,0009; the ſum ,012, when both conſpire, not exceeding much the hundredth part of a ſecond, may ſtill be rejected as inſenſible. Let us alſo examine how near the horizon it may be ſafe to obſerve ſuch ſtars. At 10°, for inſtance, the refraction is 5′ 14″,6; the difference for 5″ is ,0388; the joint effect of the changes in the barometer and thermometer is ,0034; the ſum of the whole together amounts to ,0422, which

is leſs than half the tenth of a ſecond: now this may either be taken into conſideration, or ſuch low obſervations may be avoided, as being by no means neceſſary, and but ill ſuiting the high powers a teleſcope proper for this purpoſe ought to bear.

The change of poſition of the earth's axis I look upon as an unſurmountable obſtacle to taking the parallax of ſtars by the method of zenith diſtances: for though refraction is much reduced in the zenith, this change is there no leſs ſenſible than in other parts of the heavens; but as this will always affect our two ſtars exactly alike, we are entirely freed from this embarraſſment.

The aberration of light can have no influence of the leaſt conſideration upon our two ſtars, as a mere inſpection of the tables will ſhew. In a whole degree, its effects, when greateſt, amount but to four-tenths of a ſecond, and conſequently in 5″ to no more than ,0005, or the two thouſandth part of a ſecond.

Obſervations of the relative diſtance of the two ſtars that make up a double ſtar, being thus cleared of every impediment, are capable of being continually improved by every degree of perfection the teleſcope may acquire: we can chuſe ſtars that may be viewed ſufficiently high to be clear of the vapours that ſwim near the horizon, and conſequently employ the greateſt powers our inſtruments are capable of. From experience I can alſo affirm, that the ſtars will bear a much higher degree of magnifying than other celeſtial objects. Too much has hitherto been taken for granted in optics: every natural philoſopher is ready enough to allow the neceſſity of making experiments, and tracing out the ſteps of nature; why this method ſhould not be more purſued in the art of ſeeing

does not appear. Theories are only to be used when proper data are assigned; but the data are carefully to be re-examined, when new improvements may widely alter the result of former experiments. Thus, we are told, that we gain nothing by magnifying *too much*. I grant it; but shall never believe I magnify too much till by experience I find, that I can see better with a lower power. Nor is even that sufficient: a lower power may shew more of the object; it may shew it brighter, nay even distincter, and therefore upon the whole better; and yet the greater power may, in a particular case, be preferable: for if the object is so small as not to be at all visible with the lower power, and I can, by magnifying more, obtain a view of it, though neither so bright nor distinct as I could wish, is it not evident, that here this power is preferable to the former?

The naturalist does not think himself obliged to account for all the phænomena he may observe; the astronomer and optician may claim the same privilege. When we increase the power we lessen the light in the inverse ratio of the square of the power; and telescopes will, in general, discover more small stars the more light they collect; yet with a power of 227 I cannot see the small star near the star following σ Aquilæ, when, by the same telescope, it appears very plainly with the power of 460: now, in the latter case, the power being more than double, the light is less than the fourth part of the former. In such particular cases I generally suspect my own eyes, and have recourse to those of my friends. I had the pleasure of shewing this star to Dr. WATSON junior, who soon discovered the small star, which accompanies the other, with the power of 460; but saw nothing of it with 227, though the place where to look for it had been pointed out to him by the higher power. The experiment has been too often

repeated

repeated to be doubtful, and has alſo been confirmed by others of nearly the ſame nature: for inſtance, the ſmalleſt of the two that accompany the ſtar near *k* Aquilæ, the ſmall ſtar near *μ* Herculis, and the ſmall ſtar near *α* Lyræ, are inviſible with my power of 227, and viſible with the ſame aperture when the power is 460. Alſo the ſmall ſtars near FLAMSTEAD's 24th of Aquila, the ſmalleſt of two near *σ* Coronæ, the ſmall ſtar near the ſtar ſouth of *ε* Aquilæ, the ſmall ſtar near the ſecond *o* Perſei, the ſmall ſtar near the ſtar which accompanies FLAMSTEAD's 10th ſub pede et ſcapula dextra Tauri, the ſmall ſtar, near *β* Delphini, and the ſmall ſtar near the pole ſtar, are all much brighter and ſtronger, and therefore much ſooner ſeen with 460 than with 227.

Great power may alſo, in particular circumſtances, be favourable, even with an exceſs of aberration. When two ſtars are ſo cloſe together as to make the ſcale for meaſuring the diſtance of their centers too ſmall, if, by magnifying much, we can enlarge that diſtance, we may gain a conſiderable advantage, provided the centers or apparent bodies of the ſtars remain diſtinct enough for the purpoſe of theſe meaſures. The appearance of *α* Lyræ in my Newtonian reflector with a power of 460 is repreſented in fig. 2.; with 2010 in fig. 3.; with 3168 in fig. 4.; and with 6450 in fig. 5. Now in all theſe figures we ſee, that the centers are ſtill diſtinct enough to meaſure their diſtances with ſufficient truth; or if any little error ſhould be introduced by the magnitude of the central point, it will be more than ſufficiently balanced by the largeneſs of the ſcale. In this manner, with a power of 3168, I have obtained a ſcale of no leſs than ten inches ſix tenths for the diſtance of the centers of the two ſtars of *α* Geminorum; and as we

know theſe centers to be but a few ſeconds diſtant, it is plain how great an advantage we gain by ſuch an enlarged ſcale.

Theſe experiments have but very lately pointed out to me a method of making a new micrometer, upon a conſtruction entirely different from any that are now in uſe, which I have been ſucceſsful enough to put in practice, and by which I have already begun to determine the diſtance of the centers of ſome of the moſt remarkable double ſtars to a very great degree of accuracy *.

The powers that may be uſed upon various double ſtars are different, according to their relative magnitudes: ε Bootis, for inſtance, will not bear the ſame power as α Geminorum, nor would it be difficult to aſſign a reaſon for it; but as I here ſhall merely confine myſelf to facts, it will be ſufficient in general to mention, that two ſtars, which are equal, or nearly ſo, will bear a very high power: with α Geminorum I have gone as far as 3168; but with the former only to 2010. The difficulty of uſing high powers is exceedingly great; for the field of view takes in leſs than the diameter of the hair or wire in the finder, and the effect of the earth's diurnal motion is ſo great, that it requires a great deal of practice to find the object, and manage the inſtrument. It appears to me very probable, that the diurnal motion of the earth will be the greateſt obſtacle to our progreſs in magnifying, except we can introduce a proper mechaniſm to carry our teleſcopes in a contrary motion.

Notwithſtanding opticians have proved that two eye-glaſſes will give a more correct image than one, I have always (from experience) perſiſted in refuſing the aſſiſtance of a ſecond glaſs, which is ſure to introduce errors greater than thoſe we would correct. Let us reſign the double eye-glaſs to thoſe who view objects

* For a deſcription of this micrometer ſee a ſubſequent paper.

merely

merely for entertainment, and muſt have an exorbitant field of view. To a philoſopher this is an unpardonable indulgence. I have tried both the ſingle and double eye-glaſs of equal powers, and always found that the ſingle eye-glaſs had much the ſuperiority in point of light and diſtinctneſs. With the double eye-glaſs I could not ſee the *belts on Saturn*, which I very plainly ſaw with the ſingle one. I would, however, except all thoſe caſes where a large field is abſolutely neceſſary, and where power joined to diſtinctneſs is not the ſole object of our view.

The application of the different powers of a teleſcope in general is of ſome conſequence; and in anſwer to thoſe who may think I have ſtrained or over-charged mine, I muſt obſerve, that a ſingle glance at the ſubſequent *h* Draconis, *η* Coronæ, and the ſtar near *μ* Bootis, with a power of 460, ſhewed them to me as double ſtars; when, in two former reviews of the heavens, I had twice ſet them down in my journal as ſingle ſtars, where I uſed only the power of 222 and 227, and in all probability ſhould never have found them double, had I not looked with a higher power.

We are to remember, that it is much eaſier to ſee an object when it is pointed out to us than when it falls in our way unexpectedly, eſpecially if of ſuch a nature as to require ſome attention to be ſeen at all; but to ſay no more of other advantages of high powers, it is evident, that in the reſearch of the parallax of the fixed ſtars they are abſolutely neceſſary. If we would diſtinctly perceive and meaſure or eſtimate extremely ſmall quantities, ſuch as a tenth of a ſecond, it appears, that when we uſe a power of 460, this tenth of a ſecond will be no more in appearance than 46″, and even with a power of 1500 will be but 2′ 30″, which is a quantity not much more

than

than ſufficient to judge well of objects and diſtinguiſh them from each other, ſuch as a circle from a ſquare, triangle, or polygon *.

It has been obſerved, that objects grow indiſtinct when the principal optic pencil at the eye becomes leſs than the 40th or 50th part of an inch in diameter. In the experiments that have been made upon this ſubject it appears to me, that the indiſtinctneſs which is aſcribed to the ſmallneſs of the optical pencil may be owing to very different cauſes: at leaſt it will be eaſy to bring contrary experiments of extremely ſmall pencils, not at all affected by this inconvenience; for inſtance, it is well known, that microſcopes, conſiſting of a ſingle lens or globule, are remarkable for diſtinctneſs. We alſo know, that they have been made ſo ſmall as to magnify above 10,000 times †. From this we may infer that their apertures, and conſequently the diameters of the optic pencil at the eye could not exceed the 2500th part of an inch. I am therefore inclined to believe, that we muſt look for diſtinctneſs in the perfection of the object-ſpeculum or object-glaſs of a teleſcope; and if we can make the firſt image in the focus of a ſpeculum almoſt as perfect as the real object, what ſhould hinder our magnifying but the want of light? Now, if the object has light ſufficient, as the ſtars moſt undoubtedly have, I ſee no reaſon why we ſhould limit the powers of our inſtruments by any theory. Is it not beſt to have recourſe to experiments to find

* By a ſet of experiments, made in the year 1774, I found, that I could diſcover or perceive a bright object, ſuch as white paper, againſt the ſky-light, when it ſubtended an angle of 35″; but could only diſtinguiſh it to be a circle, and no other figure, when it appeared under an angle of 2′ 24″.

† See Padre DELLA TORRE's Method, &c. Scelta di Opuſculi.

how

how far our endeavours to render the first image perfect have been successful.

As soon as I was fully satisfied that in the investigation of parallax the method of double stars would have many advantages above any other, it became necessary to look out for proper stars. This introduced a new series of observations. I resolved to examine every star in the heavens with the utmost attention and a very high power, that I might collect such materials for this research as would enable me to fix my observations upon those that would best answer my end. The subject has already proved so extensive, and still promises so rich a harvest to those who are inclined to be diligent in the pursuit, that I cannot help inviting every lover of astronomy to join with me in observations that must inevitably lead to new discoveries. I took some pains to find out what double stars had been recorded by astronomers; but my situation permitted me not to consult extensive libraries, nor indeed was it very material: for as I intended to view the heavens myself, Nature, that great volume, appeared to me to contain the best catalogue upon this occasion. However, I remembered that the star in the head of Castor, that in the breast of the Virgin, and the first star in Aries, had been mentioned by CASSINI as double stars. I also found the Nebula in Orion was marked in HUGEN's *Systema Saturnium* as containing seven stars, three of which (now known to be four) are very near together. With this small stock I begun, and in the course of a few years observations have collected the stars contained in my catalogue. I find, with great pleasure, that a very excellent observer, whom I have the honour to call my friend*, has also, though un-

* Phil. Trans. for the year 1781, part II. double stars discovered in 1779, at Frampton-house, Glamorganshire, by NAT. PIGOTT, Esq. F. R. S. &c.

known to me, met with three of those stars that will be found in my catalogue: and upon this occasion I also beg leave to observe, that the Astronomer Royal, when I was at Greenwich last May, with his usual politeness, shewed me, among other objects, α Herculis as a double star, which he had discovered some years ago. The rev. Mr. HORNSBY also, when I had the pleasure of seeing him at Oxford, in a conversation on the subject of the stars of the first magnitude that have a proper motion, mentioned π Bootis as a double star. It is a little hard upon young astronomers to be obliged to discover *over-again* what has already been discovered; however, the pleasure that attended the view when I first saw these stars has made some amends for not knowing they had been seen before me.

If I should mention in my list of observations a few that may be found difficult to be verified by other telescopes, I must beg the indulgence of the observers. I hope it will sufficiently appear, that I have guarded against optical delusions; and every astronomer, I make no doubt, will find, by those observations that fall within the compass of his instruments, and attention to circumstances necessary to the right management of them, that I have had all along truth and reality in view, as the sole object of my endeavours; and therefore he will be inclined to give some credit to what he does not immediately perceive, when he finds himself successful where he takes the proper precautions so necessary in delicate observations, even with the best instruments.

I have been in some doubt in what manner to communicate these observations. My first view was to have methodized them properly; but I find them so extensive that there is but little probability that one person should be able to bring them to a conclusion, for which reason I have now resolved to give them

unfiniſhed as they are, that every perſon who is inclined to engage in this purſuit may become a fellow-labourer.

In ſettling the diſtances of double ſtars I have occaſionally uſed two different ways. Thoſe that are extremely near each other may be eſtimated by the eye, in meaſures of their own apparent diameters. For this purpoſe their diſtance ſhould not much exceed two diameters of the largeſt, as the eye cannot ſo well make a good eſtimation when the interval between them is greater. This method has often the preference to that of the micrometer: for inſtance, when the diameter of a ſmall ſtar, perhaps not equal to half a ſecond, is double the vacancy between the two ſtars. Here a micrometer ought to meaſure tenths of ſeconds at leaſt, otherwiſe we could not, with any degree of confidence, rely on its meaſures; nay, even then, if the ſtars are ſituated in the ſame parallel of declination and near the equator, their quick motion acroſs the micrometer makes it extremely difficult to meaſure them, and in that caſe an eſtimation by the eye is preferable to any other meaſure; but this requires not a little practice, precaution, and time, and yet with proper care it will be found that this method is capable of great exactneſs. Let two ſmall circles be drawn either equal or unequal, at a diſtance not exceeding twice the diameter of the largeſt; let theſe be ſhewn to ſeveral perſons in the ſame light and point of view. Then, if every one of them will ſeparately and carefully write down his eſtimation of the interval between them, in the proportion of either of their diameters, it will be found upon a compariſon that there will ſeldom be ſo much as a quarter of a diameter difference between all the eſtimations. If this agreement takes place with ſo many different eyes, much more may we expect it in the

eſtimations of the ſame eye when accuſtomed to this kind of judgement.

I have divided the double ſtars into ſeveral different claſſes. In the firſt I have placed all thoſe which require indeed a very ſuperior teleſcope, the utmoſt clearneſs of air, and every other favourable circumſtance to be ſeen at all, or well enough to judge of them. They ſeemed to me on that account to deſerve a ſeparate place, that an obſerver might not condemn his inſtrument or his eye if he ſhould not be ſucceſsful in diſtinguiſhing them.

As theſe are ſome of the fineſt, moſt minute, and moſt delicate objects of viſion I ever beheld, I ſhall be happy to hear that my obſervations have been verified by other perſons, which I make no doubt the curious in aſtronomy will ſoon undertake. I ſhould obſerve, that ſince it will require no common ſtretch of power and diſtinctneſs to ſee theſe double ſtars, it will therefore not be amiſs to go gradually through a few preparatory ſteps of viſion, ſuch as the following: when η Coronæ borealis (one of the moſt minute double ſtars) is propoſed to be viewed, let the teleſcope be ſome time before directed to α Geminorum, or if not in view to either of the following ſtars, ζ Aquarii, μ Draconis, ρ Herculis, α Piſcium, or the curious double-double ſtar ε Lyræ. Theſe ſhould be kept in view for a conſiderable time, that the eye may acquire the habit of ſeeing ſuch objects well and diſtinctly. The obſerver may next proceed to ξ Urſæ majoris, and the beautiful treble ſtar in Monoceros's right fore-foot; after theſe to i Bootis, which is a fine miniature of α Geminorum, to the ſtar preceding α Orionis, and to π Orionis. By this time both the eye and the teleſcope will be prepared for a ſtill finer picture, which is η Coronæ borealis. It will be in vain to attempt this latter if all the former, at leaſt

i Bootis, cannot be distinctly perceived to be fairly separated because it is almost as fine a miniature of *i* Bootis as that is of *α* Geminorum. If the observer has been successful in all these, he may then, at the same time, try *b* Draconis, though I question whether any power less than 4 or 500 will shew it to be double; but the former I have all seen very well with 227.

To try the stars of unequal magnitudes it will be expedient to take them in some such order as the following: *α* Herculis, *ω* Aurigæ, *δ* Geminorum, *k* Cygni, *ε* Persei, and *b* Draconis; from these the observer may proceed to a most beautiful object, *ε* Bootis, which I have closely attended these two years as very proper for the investigation of the parallax of the fixed stars.

It appears, from what has been said, that these double stars are a most excellent way of trying a telescope; and as the foregoing remarks have suggested the method of seeing how far the power and distinctness of our instruments will reach, I shall add the way of finding how much light we have. The observer may begin with the pole-star and *α* Lyræ; then go to the star south of *ε* Aquilæ, the treble star near *k* Aquilæ, and last of all to the star following *o* Aquilæ. Now, if his telescope has not a great deal of good distinct light, he will not be able to see some of the small stars that accompany them.

In the second class of double stars I have put all those that are proper for estimations by the eye or very delicate measures of the micrometer. To compare the distances with the apparent diameters the power of the telescope should not be much less than 200, as they will otherwise be too close for the purpose. The instrument ought, moreover, to be as much as possible free from rays that surround a star in common telescopes, and should give the apparent diameters of a double star perfectly round and well-defined, with a deep black division between

between them, as in fig. 6. which represents α Geminorum as I have often seen it with a power of 460. It will be necessary here to take notice, that the estimations made with one telescope cannot be applied to those made with another: nor can the estimations made with different powers, though with the same telescope, be applied to each other. Whatever may be the cause of the apparent diameters of the stars, they are certainly not of equal magnitude with the same powers in different telescopes, nor of proportional magnitude with different powers in the same telescope. In my instruments I have ever found less diameter in proportion the higher I was able to go in power, and never have I found so small a proportional diameter as when I magnified 6450 times*; therefore if we would wish to compare any such observations together, with a view to see whether a change in the distance has taken place, it should be done with the very same telescope and power, even with the very same eye-glass or glasses; for others, though of equal power and goodness, would most probably give different proportional diameters of the stars.

In the third class I have placed all those double stars that are more than five but less than 15″ asunder; and for that reason, if they should be used for observations on the parallax of the fixed stars, they ought not to be looked upon as quite free from the effects of refraction, &c. In the same manner that the stars in the first and second classes will serve to try the goodness of the most capital instruments, these will afford objects for telescopes of inferior power, such as magnify from 40 to 100 times. The observer may take them in this or the like order: ζ Ursæ majoris, γ Delphini, γ Arietis, π Bootis, γ Virginis,

* See the measures of the diameter of α Lyræ, Catalogue of double stars, 5th class.

ginis, ι Caſſiopeæ, μ Cygni. And if he can ſee all theſe, he may paſs over into the ſecond claſs, and direct his inſtrument to ſome of thoſe that were pointed out as objects for the very beſt teleſcopes, where, I ſuppoſe, he will ſoon find the want of ſuperior power.

The fourth, fifth, and ſixth claſſes contain double ſtars that are from 15 to 30″, from 30″ to 1′, and from 1′ to 2′ or more aſunder. Though theſe will hardly be of any ſervice for the purpoſe of parallax, I thought it not amiſs to give an account of ſuch as I have obſerved; they may, perhaps, anſwer another very important end, which alſo requires a great deal of accuracy, though not quite ſo much as the inveſtigation of the parallax of the fixed ſtars. I will juſt mention it, though foreign to my preſent purpoſe. Several ſtars of the firſt magnitude have already been obſerved, and others ſuſpected, to have a proper motion of their own: hence we may ſurmiſe, that our ſun, with all its planets and comets, may alſo have a motion towards ſome particular part of the heavens, on account of a greater quantity of matter collected in a number of ſtars and their ſurrounding planets there ſituated, which may perhaps occaſion a gravitation of our whole ſolar ſyſtem towards it. If this ſurmiſe ſhould have any foundation, it will ſhew itſelf in a ſeries of ſome years; as from that motion will ariſe another kind of hitherto unknown parallax *, the inveſtigation of which may account for ſome part of the motions already obſerved in ſome of the principal ſtars; and for the purpoſe of determining the direction and quantity of ſuch a motion, accurate obſervations of the diſtance of ſtars that are near enough to be meaſured with a micrometer, and a very high power of

* See the note in the rev. Mr. MITCHELL's paper on the Parallax of the Fixed Stars, Phil. Tranſ. vol. LVII. p. 252.

teleſcopes,

teleſcopes may be of conſiderable uſe, as they will undoubtedly give us the relative places of thoſe ſtars to a much greater degree of accuracy than they can be had by tranſit inſtruments or ſectors, and thereby much ſooner enable us to diſcover any apparent change in their ſituation occaſioned by this new kind of ſyſtematical parallax, if I may be allowed to uſe that expreſſion, for ſignifying the change ariſing from the motion of the whole ſolar ſyſtem.

I ſhall now endeavour to deliver a theory of the annual parallax of double ſtars, with the method of computing from thence what is generally called the parallax of the fixed ſtars, or of ſingle ſtars of the firſt magnitude, ſuch as are neareſt to us. It may be obſerved, that the principles upon which I have founded the following theory are of ſuch a nature, that they cannot be ſtrictly demonſtrated, in conſequence of which they are only propoſed as poſtulata, which have ſo great a probability in their favour, that they will hardly be objected to by thoſe who are in the leaſt acquainted with the doctrine of chances.

GENERAL POSTULATA.

1. Let the ſtars be ſuppoſed, one with another, to be about the ſize of the ſun *.

2. Let the difference of their apparent magnitudes be owing to their different diſtances, ſo that a ſtar of the ſecond, third,

* See Mr. MICHELL's Inquiry into the probable Parallax and Magnitude of the Fixed Stars, Phil. Tranſ. vol. LVII. p. 234. 236. 237. 240. and Dr. HALLEY on the Number, Order, and Light, of the Fixed Stars, Phil. Tranſ. vol. XXXI.

or

or fourth magnitude is two, three, or four times as far off as one of the first *.

In fig. 7. let OE be the whole diameter of the earth's annual orbit; and let *a*, *b*, *c*, be three stars situated in the ecliptic, in such a manner that they may be seen all in one line O*abc*, when the earth is at O. Let the line O*abc* be perpendicular to OE, and draw PE parallel to *c*O. Then, if O*a*, *ab*, *bc*, are equal to each other, *a* will be a star of the first magnitude, *b* of the second, and *c* of the third. Let us now suppose the angle O*a*E, or parallax of the whole orbit of the earth, to be 1″ of a degree: then we have P*a*E = O*a*E = 1″.: and, because very small angles, having the same subtense OE, may be taken to be in the inverse ratio of the lines O*a*, O*b*, O*c*, &c. we shall have O*b*E = ½″, O*c*E = ⅓″, &c. †. Now, when the earth is removed

* The apparent magnitude is here taken in a stricter sense than is generally used; and by it is rather meant the order into which the stars *ought to be* distinguished than that into which they *are* commonly divided: for as the order of the magnitudes is here to denote the different relative distances, we are to examine carefully the degree of light each star is accurately found to have: and considering then that light diminishes in the inverse ratio of the squares of the distances, we ought to class the stars accordingly. An allowance ought also perhaps to be made for some loss that may happen to the light of very remote stars in its passage through immense tracts of space, most probably not quite destitute of some very subtle medium. This conjecture is suggested to us by the colour of the very small telescopic stars, for I have generally found them red, or inclining to red; which seems to indicate, that the more feeble and refrangible rays of the other colours are either stopped by the way, or at least diverted from their course by accidental deflections.

† This proves what I have before remarked on the parallax of γ Draconis; for that star, (admitting it to be a star of between the second and third magnitude, which ought to be ascertained by experiments, as mentioned in the note above) by the postulata, will have its place assigned somewhere between *b* and *c*, and therefore its parallax will be between ½ and ⅓ of the parallax of a star of the first magnitude. And if Dr. BRADLEY thought that he should have perceived a

removed to E, we ſhall have $PEb = EbO = \frac{1}{2}''$, and $PEa - PEb = aEb = \frac{1}{2}''$; that is, the ſtars a, b, will appear to be $\frac{1}{2}''$ diſtant. We alſo have $PEc = EcO = \frac{1}{4}''$, and $PEa - PEc = aEc = \frac{1}{4}''$; that is, the ſtars a, c, will appear to be $\frac{1}{4}''$ diſtant, when the earth is at E. Now, ſince we have $bEP = \frac{1}{2}''$, and $cEP = \frac{1}{4}''$, therefore $bEP - cEP = bEc = \frac{1}{2}'' - \frac{1}{4}'' = \frac{1}{8}''$; that is, the ſtars b, c, will appear to be only $\frac{1}{8}''$ removed from each other, when the earth is at E.

From what has been ſaid, we may gather the following general expreſſion, to denote the parallax that will become viſible in the change of diſtance between the two ſtars, by the removal of the earth from one extreme of its orbit to the other. Let P expreſs the total parallax of a fixed ſtar of the firſt magnitude, M the magnitude of the largeſt of the two ſtars, m the magnitude of the ſmalleſt*, and p the partial parallax to be obſerved by the change in the diſtance of a double ſtar; then will $p = \frac{m - M}{Mm} P$; and p being found by obſervation will give us $P = \frac{pMm}{m - M}$. An example or two will explain this ſufficiently. Suppoſe a ſtar of the firſt magnitude ſhould have a ſmall ſtar of the twelfth magnitude near it; then will the partial parallax

parallax in γ Draconis, if at moſt it had amounted to 2″, it follows, that the angle OaE may nearly amount to 4 or 5″ for any thing we can conclude to the contrary from thoſe obſervations.

* As M and m are here taken to expreſs the relative diſtances of the ſtars, in meaſures whereof the diſtance of the neareſt ſtar is taken as unity, thoſe who think the poſtulata on which theſe eſtimations are built cannot be granted, may ſtill uſe the following formulæ, if inſtead of the magnitudes M, m, they put their own eſtimations of the relative diſtances of the ſtars, according to any other method whatever they may think it moſt eligible to adopt; for the apparent magnitude of ſtars is here only propoſed as the moſt probable means we have of forming any conjectures about their relative diſtances.

we are to expect to see be $\frac{12 \times 1}{12-1} P$; or $\frac{1}{11}$ths of the total parallax of a fixed ſtar of the firſt magnitude; and if we ſhould, by obſervation, find the partial parallax between two ſuch ſtars to amount to 1″, we ſhall have the total parallax $P = \frac{1 \times 1 \times 12}{12-1} = 1'',0909$. If the ſtars are of the third and twenty-fourth magnitude, the partial parallax will be $\frac{24-3}{3 \times 24} = \frac{21}{72} P$; and if, by obſervation, p is found to be a tenth of a ſecond, the whole parallax will come out $\frac{1 \times 3 \times 24}{24-3} = 0'',3428$.

It will be neceſſary to examine ſome different ſituations. Suppoſe the ſtars, being ſtill in the ecliptic, to appear in one line, when the earth is in any other part of its orbit between O and E; then will the parallax ſtill be expreſſed by the ſame algebraic form, and one of the maxima will ſtill lie at O, the other at E; but the whole effect will be divided into two parts, which will be in proportion to each other as radius − ſine to radius + ſine of the ſtars diſtance from the neareſt conjunction or oppoſition.

When the ſtars are any where out of the ecliptic ſituated ſo as to appear in one line O*abc* at rectangles to OE, the maximum of parallax will ſtill be expreſſed by $\frac{m-M}{Mm} P$; but there will ariſe another additional parallax in the conjuction and oppoſition, which will be to that which is found 90° before or after the ſun, as the ſine (S) of the latitude of the ſtars ſeen at O is to radius (R); and the effect of this parallax will be divided into two parts; half of it lying on one ſide of the large ſtar, the other half on the other ſide of it. This latter parallax, moreover, will be compounded with the former, ſo that

the diſtance of the ſtars in the conjunction and oppoſition will then be repreſented by the diagonal of a parallelogram, whereof the two ſemi-parallaxes are the ſides; a general expreſſion for which will be $\sqrt{\left|\frac{m-M}{2Mm}P\right|^2 \times \frac{SS}{RR}+1}$: for the ſtars will apparently deſcribe two ellipſes in the heavens, whoſe tranſverſe axes will be to each other in the ratio of M to m (fig. 8.), and Aa, Bb, Cc, Dd, will be cotemporary ſituations. Now, if bQ be drawn parallel to AC, and the parallelogram bqBQ compleated, we ſhall have bQ $= \frac{1}{4}$CA $- \frac{1}{4}ca = \frac{1}{4}Cc = \frac{1}{4}p$, or ſemi-parallax 90° before or after the ſun, and Bb may be reſolved into, or is compounded of, bQ and bq; but $bq = \frac{1}{4}$BD $- \frac{1}{4}bd =$ the ſemi-parallax in the conjunction or oppoſition. We alſo have R : S :: bQ : $bq = \frac{pS}{2R}$; therefore the diſtance Bb (or Dd) $= \sqrt{\left|\frac{p}{2}\right|^2 + \left|\frac{pS}{2R}\right|^2}$; and by ſubſtituting the value of p into this expreſſion we obtain $\sqrt{\left|\frac{m-M}{2Mm}P\right|^2 \times \frac{SS}{RR}+1}$, as above. When the ſtars are in the pole of the ecliptic, bq will become equal to bQ, and Bb will be .7071 $P\frac{m-M}{Mm}$.

Hitherto we have ſuppoſed the ſtars to be all in one line Oabc; let them now be at ſome diſtance, ſuppoſe 5″ from each other, and let them firſt be both in the ecliptic. This caſe is reſolvable into the firſt; for imagine the ſtar a, fig. 9. to ſtand at x, and in that ſituation the ſtars x, b, c, will be in one line, and their parallax expreſſed by $\frac{m-M}{Mm}P$. But the angle aEx may be taken to be equal to aOx; and as the foregoing form gives us the angles xEb, xEc, we are to add aEx, or 5″ to xEb, and we ſhall have aEb. In general, let the diſtance

tance of the ſtars be d, and let the obſerved diſtance at E be D; then will $D = d + p$, and therefore the whole parallax of the annual orbit will be expreſſed by $\frac{DMm - dMm}{m - M} = P$.

Suppoſe the two ſtars now to differ only in latitude, one being in the ecliptic, the other, for inſtance, 5″ north, when ſeen at O. This caſe may alſo be reſolved by the former; for imagine the ſtars b, c, fig. 7. to be elevated at rectangles above the plane of the figure, ſo that aOb, or aOc, may make an angle of 5″ at O: then, inſtead of the lines Oabc, Ea, Eb, Ec, EP, imagine them all to be planes at rectangles to the figure; and it will appear, that the parallax of the ſtars in longitude muſt be the ſame as if the ſmall ſtar had been without latitude. And ſince the ſtars b, c, by the motion of the earth from O to E, will not change their latitude, we ſhall have the following conſtruction for finding the diſtance of the ſtars ab, ac, at E, and from thence the parallax P. Let the triangle $ab\beta$, fig. 10. repreſent the ſituation of the ſtars; ab is the ſubtenſe of 5″, that being the angle under which they are ſuppoſed to be ſeen at O. The quantity $b\beta$ by the former theorem is found $\frac{m - M}{Mm} P$, which is the partial parallax that would have been ſeen by the earth's moving from O to E, had both ſtars been in the ecliptic; but on account of the difference in latitude it will now be repreſented by $a\beta$, the hypothenuſe of the triangle $ab\beta$: therefore, in general, putting $ab = d$, and $a\beta = D$, we have $\frac{\sqrt{DD - dd} \times Mm}{m - M} = P$. Hence D being taken by obſervation and d, M, and m, given, we obtain the total parallax.

If the ſituation of the ſtars differs in longitude as well as latitude, we may reſolve this caſe by the following method.

Let

Let the triangle $ab\beta$, fig. 11. repreſent the ſituation of the ſtars, $ab = d$ being their diſtance ſeen at O, $a\beta = D$ their diſtance ſeen at E. That the change $b\beta$ which is produced by the earth's motion will be truly expreſſed by $\frac{m-M}{Mm}P$, may be proved as before, by ſuppoſing the ſtar a to have been placed at α. Now let the angle of poſition $ba\alpha$ be taken by a micrometer*, or by any other method that may be thought ſufficiently exact; then, by ſolving the triangle $ab\alpha$, we ſhall have the longitudinal and latitudinal differences $a\alpha$ and $b\alpha$ of the two ſtars. Put $a\alpha = x$, $b\alpha = y$, and it will be $x + b\beta = aq$, whence $D = \sqrt{x + \frac{m-M}{Mm}P\Big|^2 + yy}$; and $\frac{\sqrt{D^2 - y^2 \times M^2m^2} - xMm}{m-M} = P$.

If neither of the ſtars ſhould be in the ecliptic, nor have the ſame longitude or latitude, the laſt theorem will ſtill ſerve to calculate the total parallax whoſe maximum will lie in E. There will, moreover, ariſe another parallax, whoſe maximum will be in the conjunction and oppoſition, which will be divided, and lie on different ſides of the large ſtar; but as we know the whole parallax to be exceedingly ſmall, it will not be neceſſary to inveſtigate every particular caſe of this kind; for, by reaſon of the diviſion of the parallax, which renders obſervations taken at any other time, except where it is greateſt, very unfavourable, the forms would be of little uſe.

To finiſh this theory, I ſhall only add a general obſervation on the time and place where the maxima of parallax will happen.

* The poſition of a line paſſing through the two ſtars, with the parallel of declination of the largeſt of them, may be had by the micrometer I invented for this purpoſe in the year 1779, of which a deſcription has been given in a former paper; whence, by ſpherical trigonometry, we eaſily deduce their poſition $ba\alpha$ fig. 11. with regard to the ecliptic.

When

Fig. 1.

Fig. 2.

Fig. 3.

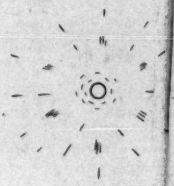


Fig. 4.

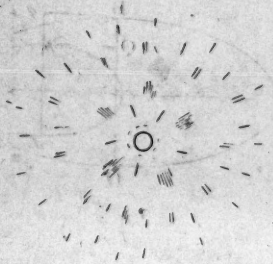


Fig. 5.

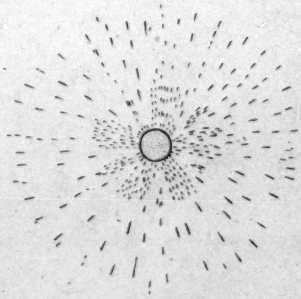


Fig. 6.

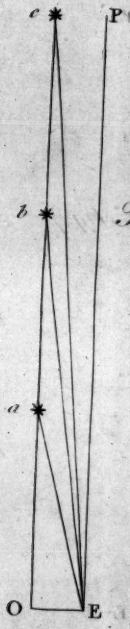
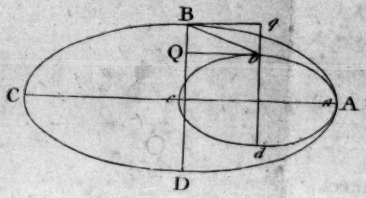


Fig. 7.

Fig. 8.



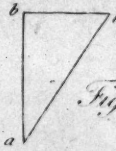


Fig. 10.

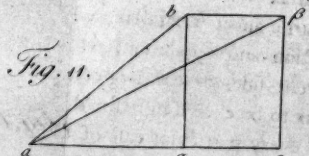
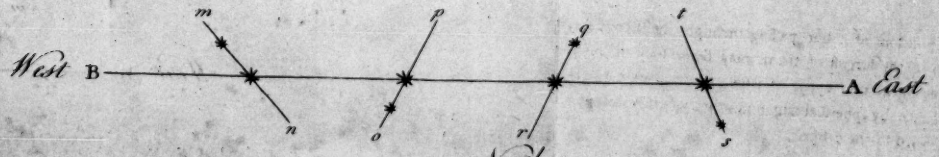


Fig. 11.

Fig. 9.

Fig. 12. South



When two unequal ſtars are both in the ecliptic, or, not being in the ecliptic, have equal latitudes, north or ſouth, and the largeſt ſtar has moſt longitude, the maximum of the apparent diſtance will be when the ſun's longitude is 90° more than the ſtars, or when obſerved in the morning; and the minimum when the longitude of the ſun is 90° leſs than that of the ſtar, or when obſerved in the evening.

When the ſmall ſtar has moſt longitude, the maximum and minimum, as well as the time of obſervation, will be the reverſe of the former.

When the ſtars differ in latitudes, this makes no alteration in the place of the maximum or minimum, nor in the time of obſervation; that is to ſay, it is immaterial whether the largeſt ſtar has the leaſt or the moſt latitude of the two ſtars.

XII. *Catalogue of Double Stars.* By *Mr.* Herſchel, *F. R. S.* *communicated by Dr.* Watſon, *Jun.*

Read January 10, 1782.

INTRODUCTORY REMARKS.

THE following catalogue contains not only double-ſtars, but alſo thoſe that are treble, double-double, quadruple, double-treble, and multiple. The particulars I have given of them are comprehended under the following general heads.

I. The names of the ſtars and number in FLAMSTEAD's Catalogue; or, if not contained therein, ſuch a deſcription of their ſituation as will be found ſufficient to point them out.

II. The comparative ſize of the ſtars. On this occaſion I have uſed the terms equal, a little unequal, pretty unequal, conſiderably unequal, very unequal, extremely unequal, and exceſſively unequal, as expreſſing the different gradations to which I have endeavoured to affix always the ſame meaning.

III. The colours of the ſtars as they appeared to me when I viewed them. Here I muſt remark, that different eyes may perhaps differ a little in their eſtimations. I have, for inſtance, found, that the little ſtar which is near α Herculis, by ſome to whom I have ſhewn it has been called green, and by others blue. Nor will this appear extraordinary when we recollect that there are blues and greens which are very often, particularly by candle-light, miſtaken for each other. The ſituation will alſo affect the

the colour a little, making a white ſtar appear pale red when the altitude is not ſufficient to clear it of the vapours. It is difficult to find a criterion of the colours of ſtars, though I might in general obſerve that Aldebaran appears red, Lyra white, and ſo on; but when I call the ſtars garnet, red, pale red, pale roſe-colour, white inclining to red, white, white inclining to blue, blueiſh white, blue, greeniſh, green, duſky, I wiſh rather to refer to the double ſtars themſelves to explain what is meant by thoſe terms.

IV. The diſtances of the ſtars are given ſeveral different ways. Thoſe that are eſtimated by the diameter can hardly be liable to an error of ſo much as one quarter of a ſecond; but here muſt be remembered what I have before remarked on the comparative appearance of the diameters of ſtars in different inſtruments. Thoſe that are meaſured by the micrometer, I fear, may be liable to an error of almoſt a whole ſecond; and if not meaſured with the utmoſt care, to near 2″. This is, however, to be underſtood only of ſingle meaſures; for the diſtance of many of them that have been meaſured very often in the courſe of two years obſervations can hardly differ ſo much as half a ſecond from truth, when a proper mean of all the meaſures is taken. As I always make the wires of my micrometer outward tangents to the apparent diameter of the ſtars, all the meaſures muſt be underſtood to include both their diameters; ſo that we are to deduct the two ſemi-diameters of the ſtars if we would have the diſtance of their centers. What I have ſaid concerns only the wire micrometers, for my laſt new micrometer is of a ſuch a conſtruction, that it immediately gives the diſtance of the centers and its meaſures (as far as in a few months I have been able to find out) may be relied on to about one-tenth of a ſecond, when a mean of three obſervations is taken. When I have

added *inaccurate*, we may ſuſpect an error of 3 or 4″. *Exactly eſtimated* may be taken to be true to about one-eighth part of the whole diſtance; but only *eſtimated*, or *about*, &c. is in ſome reſpect quite undetermined; for it is hardly to be conceived how little we are able to judge of diſtances when, by conſtantly changing the powers of the inſtrument, we are as it were left without any guide at all. I ſhould not forget to add, that the meaſure of ſtars, whereof one is extremely ſmall, muſt claim a greater indulgence than the reſt on account of the difficulty of ſeeing the wires when the field of view cannot be ſufficiently enlightened.

V. The angle of poſition of the ſtars I have only given with regard to the parallel of declination, to be reduced to that with the ecliptic as occaſion may require. The meaſures always ſuppoſe the large ſtar to be the ſtandard, and the ſituation of the ſmall one is deſcribed accordingly. Thus in figure 12. AB repreſents the apparent diurnal motion of a ſtar in the direction of the parellel of declination AB; and the ſmall ſtar is ſaid to be ſouth preceding at *mn*, north preceding at *op*, ſouth following at *qr*, and north following at *st*. The meaſure of theſe angles, I believe, may be relied upon to 2° or at moſt 3°, except when mentioned inaccurate, where an error amounting to 5° may poſſibly take place. In mere eſtimations of the angle, without any wires at all, an error may amount to at leaſt 10°, when the ſtars are near each other.

VI. The dates when I firſt perceived the ſtars to be double, treble, &c. are marked in the margin of each ſtar.

To ſhorten the work as much as poſſible, I have put L. for the large ſtar; S. for the ſmall ſtar; w. for white; r. for red; d. for duſky; n. for north; ſ. for ſouth; and have likewiſe occa-

occaſionally uſed other abbreviations that will be eaſily underſtood.

It may be ſeen, that this catalogue is yet in a very imperfect ſtate, many of the ſtars not having even the principal elements of diſtance and poſition determined with any degree of accuracy; but having already mentioned the reaſon why I give it imperfect as it is, I can only add that my endeavours will not be wanting ſoon to remove thoſe defects. However, ſince this can only be a work of ſome time, we may hope, in the mean while, that many lovers of the ſcience will turn their thoughts upon the ſame ſubject.

CATALOGUE OF DOUBLE STARS.

FIRST CLASS.

1. ε Bootis. FLAMST. 36. Ad dextrum femur in perizomate.
Sept. 9, 1779. Double. Very unequal. L. reddiſh; S. blue, or rather a faint lilac. A very beautiful object. The vacancy, or black diviſion between them, with 227 is ⅓ diameter of S.; with 460, 1¼ diameter of L.; with 932, near 2 diameters of L.; with 1159, ſtill farther; with 2010 (extremely diſtinct) 2¾ diameters of L. Theſe quantities are a mean of two years obſervation. Poſition 31° 34′ n. preceding.

2. ξ Urſæ majoris. FL. 53. In dextro poſteriore pede.
May 2, 1780. Double. A little unequal. Both w. and very bright. The interval with 222 is ⅓ diameter of L.; with 227, 1 diameter of L.; with 278, near 1¼ diameter of L. Poſition 53° 47′ f. following.

3. ν Coronæ borealis, FL. 17.

Aug. 7, 1780. Treble. The two neareſt pretty unequal; the third very faint with powers lower than 460. The two neareſt both w.; the third d. Interval of the two neareſt with 227, full 1¼ diameter of L.; with 460, 2 diameters of L. Poſition 77° 32′ n. preceding. Diſtance of the third from L. 24″ by exact eſtimation. Poſition 25° n. following by eſtimation.

4. In conſtellatione Draconis, FL. 16.

Aug. 8, 1780. Double. It is the ſtar to which a line drawn from ν through μ points, at nearly the ſame diſtance from μ as μ from ν. Conſiderably unequal. L. w.; S. w. inclining to r. With 222, 1 diameter of L.; with 278, 1¼ diameter of L. Poſition 24° 0′ f. following. There is a third ſtar, at ſome diſtance, preceding.

5. σ Caſſiopeæ, FL. 8. In dextro cubito.

Aug. 31, 1780. Double. It is the ſtar at the vertex of a teleſcopic iſoſceles triangle turned to the ſouth. Very unequal. L. w. a little inclining to r.; S. d. With 222, near 1 diameter of L.; with 460, 1¼ diameter of L. Poſition 60° 28′ n. preceding.

6. Quæ infra oculum Lyncis, FL. 12.

Oct. 3, 1780. A curious treble ſtar. Two neareſt pretty unequal. L. w.; S. w. inclining to roſe colour. With 227, about ¼ diameter; with 460, full ¼ diameter of S. Poſition 88° 37′ f. preceding. The firſt and third conſiderably unequal; ſecond and third pretty unequal. The third pale r. Diſtance from the firſt 9″ 23‴; too difficult to be extremely exact. Poſition with regard to the firſt 32° 33′ n. preceding.

7. δ Draconis,

7. *b* Draconis, FL. 39. Trium in recta, in prima inflectione colli, borea.

Oct. 3. 1780. A minute double ſtar. Extremely unequal, the ſmall ſtar being a fine lucid point. L. w.; S. inclining to r. With 227, ½ diameter of L.; with 460, full 1½ diameter of L.; with 932 (extremely fine) full 2 diameters of L. Poſition 77° 8′ n. following. A third ſtar at ſome diſtance; duſky r. Poſition 63° 22′ n. following.

8. *i* Draconis, FL. 63. In quadrilatero inflexionis primæ.

Oct. 3. 1780. A very minute double ſtar. Exceſſively unequal; the ſmall ſtar can only be ſeen when the air is perfectly clear. L. w.; S. d. With 227, leſs than 1 diameter of L.; with 278, not a diameter of L. Poſition 63° 14′ n. preceding. A pretty large third ſtar at about 3 or 4′. Poſition of this third ſtar with *i* 88° 16′ n. following.

9. In cauda Lyncis media, FL. 38.

Nov. 24. 1780. Double. Very unequal. L. w.; S. inclining to r. With 227, extremely cloſe; with 460, at leaſt ½ diameter of S. A very fine object. Poſition 25° 51′ f. preceding. A proper motion is ſuſpected in one of the ſtars.

10. In ſiniſtro anteriore pede Monocerotis, FL. 11.

Feb. 15. 1781. A curious treble ſtar; may appear double at firſt ſight; but with ſome attention we ſee that one of them again is double. The firſt, or ſingle ſtar, is the largeſt; the other two are both ſmaller, and almoſt equal, but the preceding of them is rather larger than the following. They are all w. The two neareſt with 227, 1 diameter of the preceding, or nearly

nearly 1½ of the following; with 460, 1¼ diameter of the preceding. Position of the two neareſt 11° 32′ ſ. following. For an account of the ſingle ſtar, ſee the ſecond claſs. As perfect as I have ſeen this treble ſtar with 460, it is one of the moſt beautiful ſights in the heavens; but requires a very fine evening.

11. In conſtellatione Cancri, FL. 11.

Mar. 13, 1781. Double. Conſiderably unequal. Both pale r. With 227, 1 full diameter of L.; with 460, about 1¼ diameter of L. Poſition 85° 10′ n. preceding.

12. *d* Serpentis, FL. 59. In Cauda.

July 17, 1781. Double. Very unequal. L. reddiſh w.; S. fine blue. With 227, 1 full diameter of L.; with 278, 1½ diameter of L. Poſition 44° 33′ n. preceding.

13. In conſtellatione Aquilæ, near FL. 37.

July 25, 1781. A curious treble ſtar. It is the laſt ſtar of a teleſcopic trifolium n. following *k*, ſimilar to that in the hand of Aquarius. The two neareſt very unequal; the third ſtar exceſſively ſmall, and not viſible with 227. The two neareſt with 460, no more than ¼ diameter of L.; the fartheſt about 7 or 8″.

14. In conſtellatione Aquilæ, FL. 24.

July 30, 1781. Double. In HARRIS's maps it is the ſtar in the elbow of Antinous. Exceſſively unequal; the ſmall ſtar is but juſt viſible with 227; but with 460 it is pretty ſtrong. L. pale r.; S. d. With 227, 1 full diameter of L.; with 460, 1¼ diameter of L. Poſition 72° 0′ ſ. following.

15. *i* Bootis, FL. 44.

Aug. 17, 1781. Double. In HARRIS's maps it is marked *i*, but has no letter in FL. Atlas. Conſiderably unequal. Both

w.

w. With 227 they ſeem almoſt to touch, or at moſt ¼ diameter of S. aſunder; with 460, ½ or ¾ diameter of S. This is a fine object to try a teleſcope, and a miniature of α Geminorum. Poſition 29° 54′ n. following.

16. η Coronæ borealis, FL. 2.

Sept. 9. 1781. Double. A little unequal. They are whitiſh ſtars. They ſeem in contact with 227, and though I can ſee them with this power, I ſhould certainly not have diſcovered them with it; with 460, leſs than ¼ diameter; with 932, fairly ſeparated, and the interval a little larger than with 460. I ſaw them alſo with 2010, but they are ſo cloſe that this power is too much for them, at leaſt when the altitude of the ſtars is not very conſiderable; with 460 they are as fine a miniature of *i* Bootis as that is of α Geminorum. Poſition 59° 19′ n. following.

17. In conſtellatione Bootis, near FL. 51.

Sept. 10. 1781. Double. It is a ſtar near μ not marked in FLAMSTEAD's Catalogue. Conſiderably unequal. Both duſky w. inclined to r. The interval with 460 is ¾ diameter of S. The poſition of the ſmall ſtar is turned towards μ a little following the line which joins L to μ Bootis. See μ Bootis in the ſixth claſs.

18. In conſtellatione Coronæ borealis.

Sept. 10. 1781. Double. It is the ſmalleſt of two teleſcopic ſtars between θ and δ, not contained in FL. Cat. Equal. Both d. With 460, about 1¾ diameters. Poſition 21° 0′ n. following.

19.

19. *b* Draconis, near FL. 19.

Sept. 10. 1781. One of the moſt minute of all the double ſtars I have hitherto found. It is the ſmall teleſcopic ſtar near the preceding *b* Draconis. Conſiderably unequal. Both duſky w. inclining to r. With 460, they ſeem in contact; I have however had a very good view of a ſmall dark diviſion between them. Poſition (by exact eſtimation) 25 or 30° f. preceding. They are too minute for any micrometer I have. It is in vain to look for them if every circumſtance is not favourable. The obſerver as well as the inſtrument muſt have been long enough out in the open air to acquire the ſame temperature. In very cold weather, an hour at leaſt will be required; but in a moderate temperature, half an hour will be ſufficient.

20. In dextro humero Orionis, FL. 52.

Oct. 1. 1781. Double. A little unequal. Both w. a little inclining to pale r. With 227, ½ diameter; with 460, ½ diameter. Poſition 69° 41′ f. preceding.

21. *c* Trianguli, near FL. 12. and 13.

Oct. 8. 1781. Double. It is the moſt north of a ſmall teleſcopic trapezium of unequal ſtars. Extremely unequal. With 460, ½ diameter of L. Poſition (by eſtimation) 55 or 60° n. preceding.

22. *n* Orionis, FL. 33. Duarum præcedentium 13^{m} (*a*) antecedens.

Oct. 22. 1781. Double. Conſiderably unequal. L. w.; S. w.; inclining to blue. With 227, they ſeem almoſt in contact; with 460, ½ diameter of S. Poſition 60° 55′ n. following. A very pleaſing object and eaſily ſeen.

23. In posterioribus femoribus Canis minoris.

Nov. 21, 1781. A most minute double star. It is the small telescopic star following Procyon. A little unequal. Both w. With 278, $\frac{1}{7}$ of a diameter of S.; with 460, near $\frac{1}{4}$ of a diameter of S. They are closer than η Coronæ, because their diameters, by which they are estimated, are smaller. Position 27° 21′ s. following. To see this very minute double star well, Procyon should be near its meridian altitude. There is a small telescopic star preceding the double star. Distance 1′ 59″ 39‴ from center to center.

24. ζ Cancri, FL. 16.

Nov. 21, 1781. A most minute treble star. It will at first sight appear as only a double star, but with proper attention, and under favourable circumstances, the preceding of them will be found to consist of two stars, which are considerably unequal. The largest of these is larger than the single star; and the least of the two is less than the single star. The first and second (in the order of magnitude) pretty unequal. The second and third pretty unequal. The two nearest both pale r. or r. With 278, but just separated; with 460, $\frac{1}{4}$ diameter of S. Position 86° 32′ n. following. For measures relating to the third or single star see ζ Cancri in the third class of double stars.

SECOND CLASS OF DOUBLE STARS.

1. † α Geminorum, FL. 66. In capite præcedentis II^{i}.

April 8, 1778. Double. A little unequal. Both w. The vacancy between the two ſtars, with a power of 146, is 1 diameter of S.; with 222, a little more than 1 diameter of L.; with 227, 1½ diameter of S.; with 460, near 2 diameters of L.; (ſee fig. 6.) with 754, 2 diameters of L.; with 932, full 2 diameters of L.; with 1536 (very fine and diſtinct) 3 diameters of L.; with 3168, the interval extremely large, and ſtill pretty diſtinct. Diſtance by the micrometer 5″,156. Poſition 32° 47′ n. preceding. Theſe are all a mean of the laſt two years obſervations, except the firſt with 146.

2. † α Herculis, FL. 64. In capite.

Aug. 29, 1779. A beautiful double ſtar. Very unequal. L. r.; S. blue inclining to green; the colours with every power the ſame. The interval with 222, 1¾ diameter of L.; with 227, above 2 diameters of L.; with 932, above 3 diameters of L. Diſtance 4″966. All a mean of two years obſervations. A ſingle meaſure with my laſt new micrometer, from center to center, 4″ 34‴. Poſition 30° 35′ ſ. following.

3. * ρ Herculis, FL. 75. Trium in ſiniſtro femore, tertia.

Aug. 29, 1779. Double. Pretty unequal. Both w. With 227, 1¼ diameter of L.; with 460, 2 diameters of S. Diſtance 2″,969. Poſition 30° 21′ n. preceding. The meaſures a mean of two years obſervations.

4. * ρ Serpentarii, FL. 70. Tres has ſequitur, quaſi ſupra mediam.

Aug. 29. 1779. Double. Conſiderably unequal. L. w.; S. inclining to r. With 227, 1¼ diameter of L.; with 460, much above 2 diameters of L. Poſition 9° 14′ f. following. Mean of two years obſervations.

5. et 6. * ε Lyra, FL. 4. and 5.

Aug. 29. 1779. A very curious double-double ſtar. At firſt ſight it appears double at ſome conſiderable diſtance, and by attending a little we ſee that each of the ſtars is a very delicate double ſtar. The firſt ſet conſiſts of ſtars that are conſiderably unequal. The ſtars of the ſecond ſet are equal, or the preceding of them rather larger than the following. The colour of the ſtars in the firſt ſet L. very w.; S. a little inclining to r. In the ſecond ſet both w. The interval between the ſtars of the unequal ſet, with a power of 227, is full 1 diameter of L.; with 460, near 1¼ diameter of L.; with 932, full 1¼ diameter; with 2010, 2¼ diameters. The interval between the equal ſet with a power of 227 is almoſt 1¼ diameter of either; with 460, full 1¼ diameter; with 932, 2 diameters; with 2010, 2¼ diameters. Theſe eſtimations are a mean of two years obſervations. Poſition of the unequal ſet 56° 0′ n. following. Poſition of the equal ſet 72° 57′ f. following.

7. * ζ Aquarii, FL. 55. Trium in manu dextra præcedens.

Sept. 12. 1779. Double. Equal, or the preceding rather the largeſt. Both w. With 227, 1¼ diameter; with 449, 1¼ diameter; with 460, 2 diameters; with 910, near 2 diameters; with 932, 2¼ diameters;

with 2010, pretty distinct; but too tremulous to estimate. With my 20 feet reflector, power 600, full 2 diameters, very distinct. Position, 71° 39′ n. following. Distance 4″,56, mean of two years observation.

8. ζ Coronæ borealis, FL. 7.

Oct. 1. 1779. Double. Considerably unequal. L. fine w. S. w. inclining to r. With 222, almost 3 diameters of L. Distance 5″,468. Position 25° 51′ n. preceding, mean of two years observations.

9. λ Orionis, FL. 39. In capite nebulosa.

Oct. 7. 1779. Quadruple, or rather a double star and two more at a small distance. The double star considerably unequal. L. w.; S. pale rose colour. With 222, 1½ diameter of L.; with 449, above two diameters of L. Distance 5″,833, a mean of all the measures. Position 45° 14′ n. following. As every one of the four stars is perfectly distinct, it is evident, the whole appeared nebulous to FLAMSTEAD for no other reason than because his telescope had not sufficient power to distinguish them.

10. and 11. σ Orionis, FL. 48. Ultimam cinguli præcedit ad austrum.

Oct. 7. 1779. A double-treble star, or two sets of treble stars, almost similarly situated. Preceding set. The two neareſt equal; the third larger and, compared with either of the former two, pretty unequal. The two neareſt with 222, about 2 diameters. Position of the following star of the two neareſt with the third 66° 35′ f. preceding. Position of the two neareſt, by exact estimation, 2 or 3° n. following or f. preceding

ceding the following ſet. The two neareſt very unequal. The largeſt of the two and the fartheſt conſiderably unequal. L. w.; S. blueiſh. The two neareſt with 222, about 2¼ diameters of L.; the two fartheſt 43″ 12‴. Poſition of the two neareſt 5° 5′ n. following. Poſition of the two fartheſt 29° 4′ n. following. A pretty object with 227.

12. α Piſcium, FL. ultima. In nodo duorum linorum.

Oct. 19, 1779. Double. Conſiderably unequal. Both w. With 222, not quite 2 diameters of L.; with 460, about 3 diameters of L. Diſtance 5″,123 mean meaſure. Poſition 67° 23′ n. preceding.

13. μ Draconis, FL. 21. In lingua.

Oct. 19. 1779. Double. Equal. Both w. With 227, 1¼ diameter; with 460, 2¼ diameters. Diſtance 4″,354 mean meaſure. Poſition 37° 38′ f. preceding or n. following.

14. ω Aurigæ, FL. 4.

Oct. 30, 1779. Double. Very unequal. L. w.; S. r. With 227, almoſt 2 diameters of L.; with 460, full 3 diameters of L. Poſition 82° 37′ n. preceding.

15. ψ Cygni, FL. 24. In ala dextra.

Nov. 2, 1779. Double. Extremely unequal; the ſmall ſtar a mere point. L. w.; S. r. With 227, near 1¼ diameter of L; with 278, near 1¾ diameter of L.; with 460, 2 diameters of L. Poſition 89° 32′ n. preceding.

16. ξ Cephei, FL. 17. In pectore.

Nov. 7, 1779. A fine double ſtar. Conſiderably unequal. L. w. inclining to r.; S. duſky grey. With 222, nearly 2 diameters of L. Single meaſure 5″,00. Poſition 20° 18′ n. preceding.

17. * In ſiniſtro anteriore pede Monocerotis, FL. 11.

Dec. 5, 1779. Double. With 222, about 1½ diameter. Poſition (taken Oct. 20, 1781) with the fartheſt of the other two ſtars 31° 38′ ſ. following. See the tenth ſtar in the firſt claſs.

18. ξ Bootis, FL. 37.

April 9, 1780. Double. Very unequal. L. pale r. or nearly r. S. garnet, or deeper r. than the other. With 222, 1½ diameter of L, with 460, full 3 diameters of L. Diſtance 3″ 23‴ ſingle meaſure. Poſition 65° 53′ n. following.

19. *g* Serpentarii, FL. 5.

May 2, 1780. Double. It is a ſtar in the body of Cancer, and the double ſtar is at the angular point of the three teleſcopic *g's* making a rectangle. Pretty unequal. Both w. With 227, 1½ diameter of L. Poſition 82° 10′ ſ. preceding.

20. and 21. ξ Libræ, FL. ultima.

May 23, 1710. Double double. The firſt ſet very unequal. L. fine w. With 227, nearly 2 diameters of L*. By the micrometer 6″ 23‴, but too large a meaſure. Poſition 1° 23″ n. following. The other ſet both ſmall and obſcure. With 227, perhaps 5 or 6 of their diameters aſunder.

22. ε Perſei, FL. 45. In ſiniſtro genu.

Aug. 2, 1780. Double. Extremely unequal. L. w.; S. d. With 222, 2½ diameters of L. Poſition 81° 28′ ſ. following, a little inaccurate. A third ſtar near at about 1½ or 1¾ min.

* In a future collection this ſet will be found as a treble ſtar of the firſt claſs, the large white ſtar, with a power of 460 and 932, appearing to be two ſtars.

23. In

23. In conſtellatione Serpentarii, near FL. 11.

Aug. 7, 1780. Double. It is the ſmalleſt and preceding of two in the finder. Pretty unequal. L. pale r.; S. duſky r. With 222, about 1¼ diameter of L.; with 278, about 1¾ diameter of L.; with 460, above 2 diameters of L. Poſition 46° 24′ n. preceding. A little inaccurate.

24. In conſtellatione Aquarii, FL. 108. In ſequenti flexu 5ta ad A.

Aug. 23, 1780. Double. In HARRIS's maps it is marked *i*. Unequal. With 227, 2 diameters; with 460, about 3 diameters.

25. *k* Cygni, FL. 52.

Sept. 8, 1780. Double. Extremely unequal. L. w. inclining to r.; S. d. and extremely faint; with 227, 2¼ diameters of L.; with 460, about 4 diameters of L. or more. Poſition 28° 17′ n. following.

26. In conſtellatione Orionis, near FL. 42. In longo enſis.

Oct. 23, 1780. Double. It is the moſt north of three teleſcopic ſtars in a line at the end of a cluſter near *c*. Extremely unequal. L. w.; S. d. With 278, 1¼ diameter of L. Poſition 26° 5′ n. following.

27. *δ* Geminorum, FL. 55. In inguine ſiniſtro ſequentis III.

Mar. 13, 1781. Double. Extremely unequal. L. w. inclining to r.; S. r. With 227, about 2¼ full diameters of L.; with 460, 4 or 5 diameters. Poſition 85° 51′ ſ. preceding.

28. In conſtellatione Aquilæ, near FL. 54.

July 23, 1781. Double. It is a ſtar following *o*. Exceſſively unequal. The ſmall ſtar is not viſible with 227, nor with 278. It is viſible with 460; but not without attention.

attention. Distance with 460, about 4 or 5 diameters of L. Position, by very exact estimation, 36° 28′ n. preceding.

29. In constellatione Aquilæ, near FL. 63. In medio capite.

July 31, 1781. Double. It is the star at the vertex of a telescopic isosceles triangle near r. Extremely unequal. Both r. With 460, 2 diameters of L. Position 75° 48′ n. preceding.

30. ζ Sagittæ, FL. 8. Trium in arundine sequens.

Aug. 23, 1781. Double. Extremely unequal. The small star brighter with 460 than with 227 or with 278; with 460, between 4 or 5 diameters of L.; with 278, 2½ diameters of L. Distance 5″ 27‴ inaccurate. Position 34° 10′ n. preceding.

31. In constellatione Draconis, FL. 56.

Sept. 6, 1781. Double. A little unequal. Both w. With 460, near 3 diameters. Distance 5″ 7‴.

32. In constellatione Sagittæ, near FL. 4.

Sept. 7, 1781. Double. It is the star north following s. L. pale r.; S. d. Distance 5″ 3‴ inaccurate.

33. β Orionis, FL. 19. In sinistro pede splendida.

Oct. 1, 1781. Double. Extremely unequal. L. w.; S. inclining to r. With 227, 2¼ or 2½ diameters of Rigel. With 460, more than 3 diameters of L. Distance 6″ 27‴. Position 68° 12′ s. preceding. The small star not wanting apparent magnitude is better to be seen with my power of 227 than with 460.

34. ι Trianguli, FL. 6.

Oct. 8, 1781. Double. It is marked *b* in the small triangle of HARRIS's maps. Very unequal. L. pale r. or reddish w.; S. blueish r. With 227, full 1¼ diameter

of

of L.; with 460, full 1¼ diameter of L. Position 4° 23′ n. following. A pretty object, somewhat resembling α Herculis, but smaller and not so bright.

35. In constellatione Trianguli, near FL. 6.

Oct. 8, 1781. Double. It is the star following. Equal. Both dusky w. With 460, about 2½ diameters.

36. In constellatione Eridani, FL. 32.

Oct. 22. 1781. Double. Considerably unequal. L. reddish w.; S. blue. Distance 4″ 19‴. Position 73° 23′ n. preceding.

37. In capite Monocerotis.

Oct. 22. 1781. Double. It is one of a cluster of six telescopic stars, arranged in pairs.

38. In constellatione Bootis.

Dec. 24, 1781. Double. It is the most north and largest of three in a line, s. following FL. 13. Considerably unequal, L. w.; S. inclining to r. Distance 5″ 10‴. Position 83° 3′ s. preceding.

THIRD CLASS OF DOUBLE STARS.

1. + θ Orionis, FL. 41. Trium contiguarum in longo ensis media.

Nov. 11, 1776. Quadruple. It is the small telescopic Trapezium in the Nebula. Considerably unequal. The most southern star of the following side of the Trapezium is the largest; the star in the opposite corner is the smallest; the remaining two are nearly equal. L. pale r.; the star preceding L. inclined to garnet; following L. inclined to garnet; opposite to L. d. With 460, the stars are all full, round, and well-defined.

The two ſtars in the preceding ſide diſtance 8″,780; in the ſouthern ſide, 12″,812; in the following ſide 15″,208; in the northern ſide, 20″,396.

2. ζ Urſæ majoris, FL. 59. Trium in cauda media.

Aug. 17, 1779. Double. Conſiderably unequal. L. w; S. w; inclining to pale roſe colour. Diſtance 14″,5 by two years obſervation, not a mean but that which I ſuppoſe neareſt the truth. Poſition 56° 46′ ſ. following.

3. η Caſſiopeæ, FL. 24. In cingulo.

Aug. 17, 1779. Double. Very unequal. L. fine w.; S. fine garnet, both beautiful colours. Diſtance 11″,275 mean meaſure. Poſition 27° 56′ n. following.

4. In extremitate pedis Caſſiopeæ, FL. 55. Ptolemæi.

Aug. 17, 1779. Double. Extremely unequal. L. w.; S. blueiſh r. Diſtance 7″,5 ſingle meaſure. Poſition 10° 37′ ſ. following ‡.

5. * γ Andromedæ, FL. 57. Supra pedem ſiniſtrum.

Aug. 25, 1779. Double. Very unequal. L. reddiſh w.; S. fine light ſky-blue, inclining to green. Diſtance 9″,254 a mean of two years obſervation. Poſition 19° 37′ n. following. A moſt beautiful object.

6. β Cephei, FL. 8. In cingulo ad dextrum latus.

Aug. 31, 1779. Double. Very unequal. L. blueiſh w.; S. garnet. Diſtance 13″,125. Poſition 15° 28′ ſ. preceding.

7. * β Scorpii, FL. 8. Trium in fronte, lucidarum, borea.

Sept. 19, 1779. Double. Very unequal. L. whitiſh r; S. r. Diſtance 14″,375. Poſition 64° 51′ n. following.

‡ In a future collection this will be found as a treble ſtar of the firſt claſs; the large ſtar having a ſmall one preceding, eaſily ſeen with 460 and 932.

8.

8. * π Bootis, FL. 29.
Sept. 20, 1779. Double. Pretty unequal. L. w.; S. w. inclining to r. Diſtance 6″,171. Poſition 6° 28′ ſ. following.

9. + γ Arietis, FL. 5. Quæ in cornu duarum præcedens.
Sept. 27, 1779. Double. Equal, or if any difference the following is the largeſt. Diſtance 10″,172, a mean of two years obſervation. L. w. inclining a little to r.; S. w. Poſition 86° 5′ n. preceding.

10. * γ Delphini, FL. 12. Borea ſequentis lateris, quadrilateri.
Sept. 27, 1779. Double. Nearly equal, the following a little larger. Both w. Diſtance 11″,822, being a mean of the meaſures taken in Sept. Oct. Nov. and Dec. 1779. As I ſuſpect a motion in one of theſe ſtars, I thought it beſt not to join other obſervations in that meaſure. Poſition 4° 9′ n. preceding.

11. κ Bootis, FL. 17. Trium in ſiniſtro manu præcedens.
Sept. 27, 1779. Double. Very unequal. L. w.; S. d. Diſtance 12″503, a mean of the obſervations in 1779, 80, 81. Poſition about 30° ſ. preceding.

12. ι Orionis, FL. 44. Trium contiguarum in enſe auſtrina.
Oct. 7, 1779. Treble. It is the following or largeſt of the two ι's. One is L.; the other two are extremely ſmall. L. w.; the other two both duſky r. Diſtance of the neareſt 12″,5. Diſtance of the fartheſt 48″ 31‴. Poſition of the neareſt 43° 51′ following. Poſition of the fartheſt 11° 19′ ſ. following.

13. and 14. ι Orionis, FL. 44. Trium contiguarum in enſe auſtrina.
Oct. 7, 1779. Double-treble. It is the preceding or ſmalleſt of the two ι's. The preceding ſet (forming a triangle) conſiſts of three equal ſtars. All duſky r. Diſtance

of the two neareſt, with 227, about 3 diameters. The following ſet (forming an arch) conſiſts of three ſtars of different ſizes. The middle ſtar is the largeſt; that to the ſouth is alſo pretty large; and the third is very ſmall. L. w.; l. w.; S. pale r. Diſtance 36″,25.

15. * μ Cygni, FL. 78.

Oct. 19, 1779. Double. Conſiderably unequal. L. w.; S. blueiſh. Diſtance 6″,927 mean meaſure. Poſition 20° 15′ f. following.

16. * In conſtellatione Delphini, FL. 1.

Nov. 15, 1779. Double. It is the ſtar ſouth preceding ε. A little unequal. Both w. Diſtance 12″,5. Poſition 9° 42′ f. preceding.

17. In extremitate caudæ Lacertæ, FL. 1.

Nov. 20, 1779. Double. Conſiderably unequal. L. w.; S. d. inclining to r. Diſtance 13″ 43‴ inaccurate. Poſition 76° 16′ f. preceding.

18. + γ Virginis, FL. 29. De quatuor in ala ſiniſtra, ſequens.

Jan. 21, 1780. Double. Equal. Both w. Diſtance 7″,333 mean meaſure. Poſition 40° 44′ f. following.

19. + ζ Cancri, FL. 16.

April 5, 1780. Double. Conſiderably unequal. L. pale r.; S. pale r. Diſtance 8″,046 mean meaſure. Poſition 88° 16′ f. preceding. See the 24th in the firſt claſs.

20. In conſtellatione Bootis.

June 25, 1780. Double. Draw a line through ν and ζ to the ſmall ſtar under the right foot, and erecting a perpendicular towards the left foot of equal length, the end of it will mark out this double ſtar. Pretty unequal. Both

Both r. Distance 7″ 36‴ full measure. Position 59° 32′ n. preceding.

21. In constellatione Equulei, FL 1.

Aug 2, 1780. Double. Considerably unequal. L. w.; S. much inclining to r. Distance 9″,375 mean measure. Position 5° 39′ n. following. A third small star follows at some distance.

22. Quæ infra oculum Lyncis, FL. 12.

Aug. 7, 1780. Double. With 222, about 3 diameters of L. Considerably unequal. L. w.; S. pale r. Distance 9″ 23‴, not extremely accurate. Position 32° 33′ n. preceding. See the sixth star in the first class.

23. In constellatione Cassiopeæ, FL. 34.

Aug. 8, 1780. Double. It is one of two telescopic stars, and is marked ϕ in HARRIS's maps. Extremely unequal. L. pale r.; S. d. Distance about 12″ or more.

24. θ Sagittæ, FL. 17.

Aug. 8, 1780. Treble. The two nearest extremely unequal. L. pale r.; S. d. Third star pale r. Distance of the two nearest 11″ 8‴. Distance of the two largest 1′ 7″ 49‴.

25. In constellatione Serpentarii, FL. 39.

Aug. 24, 1780. Double. It is the most south and largest of two in the finder. Very unequal. L. w.; S. inclining to blue. Distance 10″ 2‴, a little inaccurate. Position 87° 14′ n. preceding.

26. * In constellatione Cerberi 1. HEVELII 1*. FL. Herculis 95.

Sept. 8, 1780. Double. It is the star in the leaf nearest to Hercules's face and hand. Equal. Preceding w. Following,

lowing blueiſh w. Diſtance 6″ 6‴. Poſition 4° 9′ ſ. preceding or n. following.

27. In conſtellatione Navis, near FL. 3.

Feb. 15, 1781. Double. It is a ſtar between η Canis majoris and ξ Navis. Equal. Diſtance about 15″.

28. In conſtellatione Navis, near FL. 9.

Feb. 15, 1781. Double. It is one of two teleſcopic ſtars under Monoceros. Diſtance about 8″.

29. In naribus Monocerotis, FL. 8. ::

Feb. 15, 1781. Double. Diſtance about 12″.

30. * In conſtellatione Leonis, FL. 54. Duarum ſupra dorſum ſequens.

Feb. 21, 1781. Double. Conſiderably unequal. L. brilliant w.; S. aſh-colour, or greyiſh w. Diſtance 7″ 6″ mean meaſure. Poſition 9° 14′ ſ. following.

31. In conſtellatione Herculis.

May 20, 1781. Double. Over ι ::. Equal. Both very ſmall. Diſtance about 10″.

32. In conſtellatione Aquilæ, FL. 11.

July 25, 1781. Double. It is the moſt ſouth of two near ε and ζ. Exceſſively unequal. S. hardly viſible with 227, but pretty ſtrong with 460. Diſtance about 7″.

33. In conſtellatione Aquilæ, near FL. 7. and 8.

July 30, 1781. Double. It is a ſtar preceding the two ſmall ſtars north of *k* and *l*. Unequal. L. w.; S. blueiſh w. Diſtance 11″ 35‴ inaccurate, but not much.

34. In conſtellatione Aquarii, FL 94.

Aug. 20, 1781. Double. Between ψ and ω towards δ. Very unequal. Diſtance 13″ 45‴. L. pale r.; S. d.

35. In conſtellatione Serpentarii, FL. 54.

Aug. 21, Double. It is the preceding of two ſtars in the

1781. head. Exceſſively unequal. L. reddiſh w.; S. d. Diſtance about 8″.

36. In conſtellatione Perſei.

Sept. 14, 1781. Double. A little ſouth of γ. Conſiderably unequal. L. w.; S. w. inclining to r. Diſtance 11″ 53‴, rather full meaſure.

37. and 38. In conſtellatione Perſei, near FL. 38 ‡.

Sept. 24, 1781. Double-double. South preceding the firſt *o*. The equal ſet with 227, about 4 or 5 diameters. The unequal ſet about 5 or 6 diameters. Near this laſt ſet is alſo a third ſtar forming an obtuſe angle with the ſtars of this ſet. Diſtance about 10″.

39. *o* Perſei, FL. 40.

Sept. 24, 1781. Double. It is the ſecond or moſt northern *o*. Extremely unequal. L. w.; S. d. With 227, S. is hardly viſible; with 460, it appears at firſt ſight. Diſtance 14″ 59‴, inaccurate on account of the obſcurity of S.

40. In conſtellatione Herculis, near FL. 87.

Oct. 10, 1781. Double. Of three ſtars, forming an obtuſe angle, whereof FL. 87. (a ſtar ſouth of μ) is at the angular point, that towards Ramus Cereb. Extremely unequal. L. w.: S. d. Diſtance 10″ 20‴. Poſition 19° 37′ f. following.

41. * *i* Herculis, FL. 43.

Oct. 10, 1781. Double. Equal. Preceding ſtar w. A little inclined to r. Following w. Diſtance 11″ 43‴. Poſition 88° 23′ n. following.

42. In conſtellatione Trianguli.

Oct. 10, 1781. Double. It is a ſtar north following δ. Unequal. L. reddiſh, S. blueiſh. Both d. Diſtance about 6 or 7″.

‡ Mr. BRYANT of Bath firſt obſerved theſe ſtars.

43. In ſiniſtro anteriore pede Monocerotis.

Oct. 20, 1781. Double. It is the moſt ſouth of two teleſcopic ſtars preceding the treble ſtar. Extremely unequal. L. w.; S. d. Poſition 23° 39′. n. preceding.

44. In ore Monocerotis.

Oct. 20, 1781. Double. Conſiderably unequal. L. w.; S. r. Diſtance 12″ 30‴. Poſition 60° 14′ n. following.

45. In conſtellatione Tauri, near FL. 16.

Oct. 22, 1781. Double. It is near the ſtar ſub pede et ſcapula dextra. Extremely unequal. L. pale r.; S. d. Poſition 35° 33′ ſ. preceding.

46. In conſtellatione Monocerotis.

Oct. 22, 1781. Double. It is the ſtar following the tip of the ear.

FOURTH CLASS OF DOUBLE STARS.

1. α Urſæ minoris, FL. 1. Stella Polaris.

Aug. 17, 1779. Double. Extremely unequal. L. w.; S. r. Diſtance 17″ 15‴. Poſition 66° 42′ ſ. preceding.

2. * η Lyræ, FL. 20. Duarum contiguarum ad ortum a teſta, borea.

Aug. 29, 1779. Double. Conſiderably unequal. L. w.; S. r. Diſtance 25″ 42‴. Poſition 31° 51′ ſ. preceding. Three other ſtars in view.

3. ξ Capricorni, FL.

Sept. 19, 1779. Double. It is the preceding ſtar of two. Extremely unequal. Diſtance about 25″.

4. η Perſei, I. HEVELII 9. In dextro brachio.
Sept. 20, 1779. Double. Very unequal. L. r.; S. blue. Diſtance 26″, very inaccurate. Poſition 20° 5′ n. preceding.

5. In conſtellatione Arietis, FL. 33. Quatuor inform. ſup. dorſ. præc.
Sept. 27, 1779. Double. It is the firſt in the head of the fly. L. w.; S. d. Conſiderably unequal. Diſtance 25″ 32‴ inaccurate. Poſition 87° 14′.

6. + θ Serpentis, FL. 63. In extremitate Caudæ.
Oct. 17. Double. Equal. Both w. Diſtance 19″,375.

7. ψ Draconis, FL. 31. Prima ad ψ.
Oct. 19, 1779. Double. Pretty unequal. L. w.; ſ. pale r. Diſtance 28″ 14‴.

8. * ζ Piſcium, FL. 86. Trium in lino lucidarum ſequens.
Oct. 19, 1779. Double. Pretty unequal. L. w.; S. w. inclining to blue. Diſtance 22″,187, not very accurate. Poſition 22° 37′ n. following.

9. * Prima ad ψ Piſcium, FL. 74. Trium in pinna coſtarum præcedens.
Oct. 30, 1779. Double. Diſtance 27″,5. Poſition about 80° ſ. following. An obſcure ſtar alſo within 1½ minute.

10. χ Tauri, FL. 59. Auſtralis ſequentis lateris quadrilateri, in cervice.
Oct. 30. Double. Diſtance 18″,75, very inaccurate.

11. χ Cygni, FL. 17.
Nov. 20, 1779. Double. Very unequal. L. w.; S. duſky r. Diſtance 24″ 52‴.

12. * ψ Aquarii, FL. 91.
Nov. 26, 1779. Double. It is the firſt of three ψ's. Unequal. Diſtance 23″ 5‴, pretty accurate.

13. In conſtellatione Leonis, FL. 83.

April 6, 1780. Double. It is a ſmall ſtar north preceding τ. A little unequal. Both inclining to r. Diſtance 29″ 5‴. Poſition 54° 55′ ſ. following.

14. In conſtellatione Aquilæ, FL 57.

Aug. 2, 1780. Double. It is the preceding of two, near the ſouth end of Antinous's bow. A little unequal. L. w.; S. w. inclining to r. Diſtance 29″ 28‴, pretty accurate. Poſition 81° 55′ ſ. preceding.

15. In dextra aure Camelopardali. I. HEVELII ultima.

Aug. 2, 1780. Double. A little unequal. L. reddiſh w.; S. reddiſh w. Diſtance 20″ 5‴.

16. In conſtellatione Caſſiopeæ, FL. 31.

Aug. 2, 1780. Double. It is marked with the letter A in HARRIS's maps. Diſtance about 20″ or more.

17. * Cor Caroli, FL. 12. Canum Venaticorum.

Aug. 7, 1780. Double. Very unequal. L. w.; S. inclining to r. Diſtance 20″ 0‴, inaccurate. Poſition 41° 47′ ſ. preceding.

18. * In conſtellatione Cygni, FL. 61.

Sept. 20. 1780. Double. It is a ſtar preceding τ. Pretty unequal. L. pale r.; S. r.; or L. r.; S. garnet. Diſtance 16″ 7‴. Poſition 36° 28′ n. following.

19. In conſtellatione Aurigæ, FL. 14.

Sept. 24, 1780. Double. It is the preceding ſtar of a cluſter of ſtars that precede φ and χ. Very unequal. L. reddiſh w.; S. d. Diſtance 16″ 8‴, a little inaccurate. Poſition 37° 38′ ſ. preceding.

20. *o* Draconis, FL 47.

Oct. 3, 1780. Double. Very unequal. L. pale r.; S. duſky r. Diſtance 26″ 39‴. Poſition 90° n. preceding or following, by exact eſtimation.

21.

21. ζ Orionis, FL. 50. Trium in cingulo ſequens.
Oct. 10, 1780. Double. Very unequal. L. w.; S. d. Diſtance about 25″. Poſition 83° 25′ n. following, very inaccurate.

22. *f* Cygni, FL. 63. ::
Oct. 27, 1780. Double. Extremely unequal. L. fine w.; S. d. Diſtance 18″ 11‴.

23. 2 ad ω Cygni, FL. 45. In genu dextro.
Oct. 27, 1780. Double. Conſiderably unequal. L. reddiſh w.; S. d. Diſtance within 30″. Poſition 7° 23′ n. preceding.

24. 3 ad ω Cygni, FL. 46. In genu dextro.
Oct. 27, 1780. Treble. Very unequal, and extremely unequal. L. fine garnet; S. r.; ſmalleſt d. All within 30″. Poſition of the brighteſt of the two ſmall ſtars 44° 19′ n. preceding. Poſition of the fainteſt —— preceding.

25. In conſtellatione Ceti.
Dec. 23, 1780. Double. It is a ſtar near the place of the periodical ſtar *o*. Diſtance 16″,875, a little inaccurate.

26. In conſtellatione Navis, FL. 19. ::
Feb. 15, 1781. Double. It is a ſtar under the ham of Monoceros's right-foot. Diſtance about 25″.

27. In conſtellatione Comæ Berenices, FL. 24.
Feb. 28, 1781. Double. Conſiderably unequal. L. whitiſh r.; S. blueiſh r. Mean diſtance 18″ 24‴. Poſition 3° 28′ n. preceding.

28. In conſtellatione Geminorum.
Mar. 13, 1781. Double. It is near γ towards ζ Tauri. A little unequal. Both r. Diſtance 19″ 41‴. Poſition 57° 0′ f. preceding.

29. *b* Urſæ majoris, FL. 23. Duarum in collo ſequens.

Apr. 25, 1781. Double. Extremely unequal. L. reddiſh w.; S. d. Diſtance with 460, 19″ 14‴. Poſition 3° 14′ n. preceding.

30. In conſtellatione Lyncis, FL. 44.

May 26, 1781. Double. It is the eye or noſe of Leo minor. Unequal. Diſtance 24″ 53‴ inaccurate.

31. In conſtellatione Cephei, near FL. 27.

May 27, 1781. Treble. It is a ſtar near *δ*. Diſtance of the neareſt about 20″.

32. * In conſtellatione Serpentarii, FL. 61.

July 15, 1781. Double. It is a ſtar near *γ*. A little unequal. L. w.; S. grey. Diſtance 19″ 4‴, inaccurate. Poſition almoſt directly following.

33. In conſtellatione Aquilæ.

July 19, 1781. Treble. It is the firſt of two ſtars preceding *v*. Diſtance of the two neareſt 21″ 59‴, inaccurate.

34. In conſtellatione Aquilæ, near FL. 64.

July 25, 1781. Double. It is near a ſtar preceding *θ*. Equal diſtance about 30″.

35. *β* Delphini, FL. 6. Auſtrina præcedentis lateris quadrilateri.

Aug. 1, 1781. Double. Extremely unequal. Hardly viſible with 227; pretty ſtrong with 460. Diſtance 25″ 54‴, rather narrow meaſure. Poſition 78° n. preceding, by exact eſtimation.

36. *β* Serpentis, FL. 28. In eductione colli.

Aug. 13, 1781. Double. Extremely unequal. L. w.; S. extremely faint. Diſtance 24″, pretty exactly eſtimated. Poſition 3 or 4° ſ. preceding, too obſcure for meaſuring.

37.

37. δ Equulei, FL. 7. Duarum in ore ſequens.

Aug. 13, 1781. Double. Exceſſively unequal. S. hardly viſible with 227; but with 460, viſible at firſt ſight. L. w.; S. d. Diſtance 19″ 32‴. S. too obſcure to be very accurate. Poſition 11° 39′ n. following.

38. In conſtellatione Aquarii, FL. 24.

Aug. 14, 1781. Double. It is the ſtar in the cheek or hair of the neck. Very unequal. L. w.; S. d. Diſtance 25″, very inaccurate.

39. In conſtellatione Cygni.

Oct. 1, 1781. Double. It is a ſtar north following σ. Extremely unequal. L. w.; S. d. Diſtance 18″ exact eſtimation. Poſition 30° 28′ f. following.

40. *a* Trianguli, FL. 10.

Oct. 8, 1781. Double. It is the preceding of three teleſcopic ſtars. Unequal. Diſtance 17″ 19‴, pretty accurate.

41. μ Herculis, FL. 86.

Oct. 10, 1781. Double. Exceſſively unequal. The ſmall ſtar is not viſible with 227, nor with 278. I ſaw it very well with 460. L. inclined to pale r.; S. d. Diſtance, by pretty exact eſtimation, 18″. Poſition, by very exact eſtimation, 30° f. preceding.

42. In conſtellatione Herculis.

Oct. 10, 1781. Double. It is a ſtar juſt by ν. Conſiderably unequal. L. inclined to r.; S. inclined to blue. Diſtance 18″ 19‴. Poſition 4° 58′ n. preceding.

43. λ Eridani, FL. ultima. In origine fluvii.

Oct. 22, 1781. Double. It is the middle of three teleſcopic ſtars. Very unequal. L. w.; S. r.

44. In

44. In conſtellatione Tauri, near FL. 4.

Dec. 22, 1781. Double. It is a ſmall teleſcopic ſtar ſouth following *s*. Extremely unequal. L. w.; S. d.

FIFTH CLASS OF DOUBLE STARS.

1 ∂ Herculis, FL. 11. In ſiniſtro humero.

Aug. 9, 1779. Double. Extremely unequal. L. w.; S. inclining to r. Diſtance 33″,75. Poſition 72° 28′ f. following.

2. * ζ Lyræ, FL. 6.

Aug. 29, 1779. Double. Pretty unequal. L. w.; S. w. inclining to pale roſe colour. Diſtance 41″ 58‴, perhaps a little inaccurate. Poſition 62° 18′ f. following, a little inaccurate.

3. * β Lyræ, FL. 10. Duarum in jugimento borea.

Aug. 29, 1779. Quadruple. All w. Firſt and ſecond conſiderably unequal. Firſt and third very unequal. Firſt and fourth very unequal. The ſecond a little inclining to r. The third and fourth more inclining to r. Diſtance of the firſt and ſecond 43″ 57‴. Poſition 60° 28′ f. following, a little inaccurate.

4. ∂ Cephei, FL. 27. Sequitur tiaram.

Aug. 31, 1779. Double. Conſiderably unequal. L. reddiſh w.; S. blueiſh w. Diſtance 38″ 18‴, a bright object.

5. † β Cygni, FL. 6. In ore.

Sept. 12, 1779. Double. Conſiderably unequal. L. pale r.; S. a beautiful blue. The eſtimation of the colours the ſame

same with 227 and 460. Distance 39″ 32‴, pretty accurate. Position 36° 28′ n. following.

6. * ν Scorpii, FL. 14. Duarum adjacentium boreæ frontis, borea.

Sept. 19, 1779. Double. Very unequal. Both w. Distance 38″ 20‴, pretty accurate. Position 69° 28′ n. preceding.

7. μ Sagittarii, FL. 13. In summo arcu, borealis.

Sept. 19, 1779. Treble. Two small stars near on each side. L. w.; S. both r. Distance of the nearest about 30″. Position — preceding, the other — following.

8. κ Herculis, FL. 7. In dextri brachii ancone.

Sept. 20, 1779. Double. A little unequal. L. r.; S. garnet; or L. pale r.; S. r. (when the stars are low the first estimation of the colours will take place). Distance 39″ 59‴. Position 79° 37′ n. following. Has also a third star.

9. ι Bootis, FL. 21. Trium in sinistra manu, media.

Sept. 27. 1779. Double. Very unequal. L. w.; S. d. Distance 37″,56. This is not a mean of the measures; for I suspect a motion in one of the stars, which another year or two may shew. Position 52° 51′ n. following.

10. * δ Orionis, FL. 34. Trium in cingulo præcedens.

Oct. 6, 1779. Double. Considerably unequal. L. w.; S. blueish r. Distance 52″,968 full measure. Position 88° 10′ n. preceding.

11. † ν Draconis, FL. 24. and 25, In ore duplex.

Oct. 19, 1779. Double. A little unequal. L. pale r.; S. pale r. Distance 54″ 48‴. Position 44° 19′ n. preceding.

From the right ascension and declination of these stars in FLAMSTEAD's catalogue we gather, that in his

his time their diſtance was 1′ 11″,418; their poſition 44° 23′ n. preceding; their magnitude equal or nearly ſo. The difference in the diſtance of the two ſtars is ſo conſiderable, that we can hardly account for it otherwiſe than by admitting a proper motion in either one or the other of the ſtars, or in our ſolar ſyſtem; moſt probably neither of the three is at reſt.

12. * λ Arietis, FL. 9. In vertice.

Oct. 30, 1779. Double. Conſiderably unequal. L. pale r.; S. duſky garnet. Diſtance 36″ 44‴, a little inaccurate. Poſition 42° 0′ n. following.

13. φ Tauri, FL. 52. Borea ſequentis lateris quadrilateri in Cervice.

Oct. 30. Double. Diſtance 55″,625, inaccurate.

14. In conſtellatione Monocerotis.

Dec. 5, 1779. Multiple. It is a ſpot over the right fore-foot; 4 or 5 ſmall ſtars within one minute.

15. *c* Urſæ majoris, FL. 16.

May 2, 1780. Double. Very unequal. L. whitiſh r.; S. d. Diſtance with 460, 48″ 59‴. Poſition 80° 47′ ſ. preceding.

16. σ Piſcium, FL. 76. Duarum in ore piſcis ſequentis borealior.

Aug. 3, 1780. Double. Extremely unequal. L. pale r.; S. duſky r. Diſtance 48″,125, pretty accurate. Poſition 15° 28′ n. preceding.

17. π Andromedæ, FL. 29. In dextro humero.

Aug. 25, 1780. Double. Extremely unequal. L. w.; S. blueiſh. Diſtance 34″ 12‴, inaccurate.

18. α Caſſiopeæ, FL. 18. In pectore.

Aug. 31, 1780. Double. Extremely unequal. L. pale r.; S. d. Diſtance 52″,812. Poſition 40° 58′ n. preceding.

19. γ Herculis, FL. 20. In dextro brachio.

Sept. 4, 1780. Double. Extremely unequal. L. reddiſh w.; S. r. Diſtance 41″ 49‴, a little inaccurate. Poſition 19° 30′ ſ. preceding.

20. *e* Pegaſi, FL. 1.

Sept. 8, 1780. Double. Very unequal. L. pale r.; S. d.; Diſtance 37″ 5‴, pretty accurate. Poſition 38° 19′ n. preceding.

21. τ Aurigæ, FL. 29.

Sept. 26, Double, about 30″.

22. λ Aurigæ, FL. 15.

Sept. 30. Multiple. Two are within about 30″.

23. In conſtellatione Orionis.

Oct. 10, 1780. Double. It is a ſtar following *f*. Diſtance about 40″

24. In conſtellatione Ceti, FL. 37.

Oct. 12, 1780. Double. It is a ſtar between η and θ towards the north. Diſtance 42″,812, inaccurate.

25. τ Orionis, FL. 20. ſupra talum in tibia.

Oct. 29. Double. Very unequal. Diſtance about 30″

26. *b* Leonis, FL. 6.

Feb. 21, 1781. Double. Very unequal. L. r.; S. d. Diſtance 35″ 48‴. Poſition 12° 55′ n. following.

27. In conſtellatione Libræ, near FL. 31.

May. 24, 1781. Double. The moſt ſouth of three ſmall ſtars in the finder. Equal, or the preceding rather the largeſt. Both w. inclining to pale r. Diſtance 44″ 12‴, a little inaccurate. Poſition 40° 17′ ſ. following.

28. In conſtellatione Cephei.

May 27, 1781. Double. It is a ſtar near β. Extremely unequal. Diſtance about 30″.

29. ν Serpentis, FL. 53. Poſt dextrum femur Serpentarii.

July 16. Double. Unequal. Diſtance about 35″.

30. In conſtellatione Serpentarii, FL. 53.

July 19, 1781. Double. It is a ſtar between α and β one-third of the way from α. Very unequal. L. w.; S. inclining to r. Diſtance 32″ 21‴, narrow meaſure.

31. In conſtellatione Aquilæ.

July 19, 1781. Double. It is the ſtar next but one preceding δ. Very unequal. L. r.; S. d. Diſtance about 30″.

32. α Andromedæ.

July 21, 1781. Double. Extremely unequal. The ſmall ſtar better with 460 than with 227. L. w.; S. d. Diſtance 55″ 32‴, rather narrow meaſure. Poſition 10° 37′ f. preceding.

33. *h* Aquilæ, FL. 15.

July 25, 1781. Double. Unequal. Both pale r. Diſtance 33″ 53‴, inaccurate.

34. In conſtellatione Aquilæ, near FL. 28.

July 25, 1781. Double. It is one of two ſtars near A. Diſtance about 35″.

35. In conſtellatione Aquilæ.

July 25. 1781. Double. It is a ſtar near that which follows θ. Very unequal. Diſtance about 40″.

36. *o* Scuti, FL. 2. in conſtellatione Aquilæ.

July 30, 1781. Double. Very unequal. L. pale r.; S. d. Diſtance 42″ 44‴, a little inaccurate.

37.

37. υ Coronæ, FL. 18.

Sept. 21, 1781. Treble. Very unequal. L. w.; S. both r. Distance of the neareſt about 50″; the 1¼ min. ‡

38. In conſtellatione Herculis, FL. 23.

Sept. 21, 1781. Double. It is the ſtar between ν and ξ Coronæ, the largeſt of a teleſcopic triangle. Diſtance 36″ 27‴, rather narrow meaſure. L. w.; S. w. inclining to r.

39. α Lyræ, FL. 3. In teſta fulgida.

Sept. 24. 1781. Double. Exceſſively unequal. By moon-light I could not ſee the ſmall ſtar with 278, and ſaw it with great difficulty with 460; but in the abſence of the moon I have ſeen it very well with 227. L. fine brilliant w.; S. duſky. Diſtance 37″ 13‴. Poſition 26° 46′ f. following.

Oct. 22, 1781. Having often meaſured the diameters of many of the principal fixed ſtars, and having always found that they meaſured leſs and leſs the more I magnified, I fixed upon this fine ſtar for taking a meaſure with the higheſt power I have yet been able to apply, and upon the largeſt ſcale of my new micrometer I could conveniently uſe. With a power of 6450 (determined by experiments upon a known object at a known diſtance) I looked at this ſtar for at leaſt a quarter of an hour, that the eye might adapt itſelf to the object; having experimentally found, that the aberration by this means will appear leſs and leſs, and, in the teleſcope I uſed upon this occaſion with powers from 460 to 1500, will often quite vaniſh, and

‡ In a future collection the ſmall ſtar at the obtuſe angular point will be found as a double ſtar of the ſecond or third claſs.

leave a very well-defined circular disk for the apparent diameter of the stars. The diameter of α Lyræ, by this attention, appeared perfectly round, and occasionally separated from rays that were flashing about it; From the very brilliant appearance of the star with this great power, and a pretty accurate rough calculation founded on its apparent brightness, when observed with the naked eye with 227, with 460, with 6450, I surmise, that it has light enough to bear being magnified at least a hundred thousand times with no more than six inches of aperture, provided we could have such a power, and other considerations would allow us to apply it. When I had as good a view as I expected to have, I took its diameter with my new micrometer upon a scale of eight inches and 4428 ten thousandth to 1″ of a degree, and found it subtended an angle of 0″,3553. I had no person at the clock; but suppose the time of its passing through the field of my telescope (which in this great power is purposely left undefined, and as large as possible) was less than three seconds.

40. ϑ Lyræ, FL. 8.

Sept. 24, 1781. Treble. Extremely unequal. L. w.; S. both d. One n. preceding, the other s. following. Distance of the following star 56″ 47‴, a little inaccurate. Position of the same 28° 27′ s. following.

41. A Persei, FL. 43.

Sept. 24. Double. Unequal. L. w. Distance about 50″.

42. In constellatione Lyræ.

Sept. 25. Double. It is a small star just by ϑ. A little unequal

1781. equal. Both r. Distance 58″ 8‴. Position 26° 18′ n. following.

43. In constellatione Cygni, FL. 76.

Oct. 1, 1781. Double. It is the third star from ρ towards υ. Unequal. Distance 48″ by exact estimation. Position —— preceding.

44. In constellatione Cygni, FL. 69.

Oct. 1, 1781. Treble. Very unequal. L. w.; S. both reddish. Position both —— preceding.

45. In constellatione Cygni.

Oct. 1, 1781. Double. It is the most south of two telescopic stars following τ. Very unequal. L. w.; S. d. Distance 44″ by exact estimation. Position —— following.

46. *c* Cygni, FL. 16. 1ª ad *c*.

Oct. 5, 1781. Double. It is the star next following θ. Almost equal. Both pale r. Distance 30″ by pretty exact estimation.

47. *c* Cygni, FL. 26. 2ª ad *c*.

Oct. 8, 1781. Double. Very unequal. L. reddish w.; S. dusky r. Distance 39″ by pretty exact estimation.

48. * In constellatione Piscium.

Oct. 8, 1781. Double. It is a telescopic star just by θ northwards. Both d. Distance about 45″.

49. * In constellatione Arietis, FL. 30.

Oct. 15, 1781. Double. It is a small star over the Ram's back. Neatly equal. Distance 31″ 6‴, inaccurate.

50. γ Leporis, FL. 13. In posterioribus pedibus australibus.

Oct. 22, Double. Considerably unequal. Distance about 40′.

51. In constellatione Sagittæ.

Nov. 23, 1781. Double. It is a star north following 11. Extremely unequal. Distance 32″ 48‴. L. r.; S. blue.

SIXTH CLASS OF DOUBLE STARS.

1. *o* Ceti, FL. 68. In pectore nova.

Oct. 20. Double. Very unequal. L. garnet. S. dusky.

1777. Dist. { mean of some very accurate measures 1′44″,218
mean of other very accurate measures 1′53″,032.

As I can hardly doubt the motion of this star, I have given the mean of the most accurate measures separately; and hope in a few years time to be able to give a better account of it.

2. *o* Serpentarii, FL. 67.

Aug. 29, 1779. Double. Distance about 1¼ min.

3. *δ* Lyræ, FL. 11.

Aug. 29, 1779. Double. Extremely unequal. L. w. ; S. d. Distance about 4′, pretty exact estimation.

4. *π* Capricorni, FL. 5.

Sept. 19, 1779. Double. Very unequal. L. r. ; S. d. Distance about 1¼ min. Position —— f. preceding.

5. In constellatione Arietis, FL. 35. supra dorsum.

Sept. 27, 1779. Double. It is the star in the body of the fly. Distance 2′ 5″.35‴.

6. *ε* Capricorni, FL. 39. Duarum in eductione caudæ præced.

Sept. 27, 1779. Double. Unequal. L. pale r. Distance about 1¼ min.

7. ** *τ* Tauri, FL. 94. In eductione cornu borei.

Oct. 6. Double. Distance 1′ 11″,25‴, pretty accurate.

8. *κ* Tauri, FL. 59.

Oct. 6. Double. At a considerable distance.

9. * ζ Geminorum, FL. 43. In ſiniſtro genu ſequentis II^{i}.

Oct. 7, 1779. Double. Very unequal. L. reddiſh w.; S. duſky r. Diſtance 1′ 31″ 52‴, rather full meaſure. Poſition 81° 14′ n. preceding.

10. *o* Cygni, FL. 31. Duarum in dextro pede ſequens.

Nov. 2, 1779. Double. Conſiderably unequal. L. pale r. S. blue. It is the following ſtar of the two *o*'s that are cloſe together. Diſtance 1′ 39″ 57‴. Poſition 87° 14′ f. preceding.

11. * α Leonis, FL. 32. In corde.

Nov. 14, 1779. Double. Very unequal. L. w.; S. d. Diſtance 2′ 48″ 20‴. Poſition 30° 5′ n. preceding.

12. * γ Leonis, FL. 84. Quaſi in cubito.

April 6, 1780. Double. Conſiderably unequal. L. r.; S. inclining to blue. Diſtance 1′ 22″ 42‴. Poſition 73° 29′ f. following.

13. *o* Leonis, FL. 95. In extremitate caudæ.

April 6, 1780. Double. Extremely unequal. L. reddiſh w.; S. d. Diſtance about 1¼ min. Poſition about 80° n. following.

14. η Serpentis, FL. 58. In cauda.

June 19, 1780. Double. Extremely unequal. L. pale r.; S. d. Diſtance 1′ 21″ 2‴. Poſition 9° 7′ f. following.

15. In conſtellatione Bootis, near FL. 6.

June 25, 1780. Double. It is a teleſcopic ſtar near that which forms a rectangle with α and η. Diſtance about 2′.

16. δ Bootis, FL. 49. In dextro humero.

July 23, 1780. Double. Conſiderably unequal. Diſtance about 2¼ min. L. reddiſh w.; S. w. Poſition 5° 46′ n. following.

17.

17. μ Bootis, FL. 51. In baculo recurvo.

July 30, 1780. Double. Unequal. Distance 2′ 8″, exact estimation. Position 80° 25′ f. following. L. reddish w. S. pale r. See the 17th star of the first class.

18. γ Coronæ, FL. 21.

July 30, 1780. Double. Very unequal. L. r.; S. garnet. At some considerable distance. Position about 80° n. following.

19. χ Persei.

Aug 2, 1780. Multiple. An astonishing number of small stars all within the space of a few minutes. I counted not less than 40 within my small field of view.

20. μ Persei, FL. 51. Duarum in dextro poplite sequens.

Aug. 2, Double. Very unequal. L. w. Distance about 1′¼.

21. η Pegasi, FL. 44.

Aug. 23. Double. Distance about 2¼ min.

22. In constellatione Draconis, I. HEVELII 69.

Aug. 7, 1780. Double. It is the star between α Draconis and the tail of Ursa major. Distance about 3¼ min.

23. In naribus Lyncis.

Aug. 7. Double. Distance about 2′.

24. *d* Cassiopeæ, FL. 4.

Aug. 12, 1780. Treble. Two are large. Distance about 2′. A third is obscure. Distance about 1¼ min. They form almost a rectangle.

25. In constellatione Cassiopeæ, FL. 3.

Aug. 18. Double. Distance about 2¼ min.

26. ϵ Sagittæ, FL. 11.

Aug. 19, 1780. Double. Very unequal. L. r.; S. r. inclining to blue. Distance 1′ 31″ 53‴. Position 8° 32′ f. following.

27.

27. In conſtellatione Aquilæ.

Aug. 24. Double. It is a ſtar north of θ. Diſtance about 1′.

28. β Capricorni, FL. 9. Trium in ſequente cornu auſtrina.

Aug. 26, 1780. Double. Conſiderably unequal. Diſtance about 3′. Poſition —— preceding.

29. π Capricorni, FL. 10. Trium in roſtro præcedens.

Aug. 26. Double. Diſtance about 2½ min.

30. α Aurigæ, FL. 13. In humero ſiniſtro.

Sept. 8, 1780. Double. Extremely unequal. L. w.; S. d. Diſtance 2′ 49″ 8‴. Poſition 33° 42′ f. following. With a power of 227, and my common micrometer, the diameter of this ſtar meaſured 2″,5. The circumference was remarkably well defined.

31. *d* Tauri, FL. 88. In ſiniſtro cubito.

Sept. 24, 1780. Double. Diſtance 1′ 10″,625. A little inaccurate.

32. λ Cygni, FL. 54.

Sept. 20, 1780. Double. Extremely unequal. L. blueiſh w.; S. d. Diſtance about 1 min. Poſition 12° 42′ f. following.

33. In conſtellatione Cygni, FL. 32.

Sept. 20. Double. Diſtance about 2 min.

34. θ Aurigæ, FL. 37. In dextro carpo.

Sept. 26. Double. Diſtance about 2½ min.

35. In conſtellatione Camelopardali, FL. 13.

Sept. 26. 1780. Double. It is the ſtar over the goat's head. Diſtance about 2′.

36. In conſtellatione Camelopardali, FL. 10.

Sept. 30. Double. Diſtance about 1½ min.

37. *c* Draconis, FL. 46. In flexura colli.

Oct. 3. Double. Distance 3 or 4′. A rich spot.

38. *e* Draconis, FL. 64 or 65.

Oct. 3. Double. Distance about 2′.

39. *α* Orionis, FL. 58. In dextro humero lucida rutilans.

Oct. 10, 1780. Double. Extremely unequal. L. r. but not deep; S. d. Distance 2′ 6″ 2‴. Position 62° 18′ f. following.

40. *γ* Leporis, FF. 13.

Feb. 21, 1781. Double. Distance about 2½ min.

41. *ρ* Cancri 5 ad *f*, FL. 67.

Feb. 21, 1781. Double. Very unequal. L. reddish w.; S. d. Distance 1′ 35″ 59‴. Position 50° 33′ n. preceding.

42. *β* Geminorum, FL. 78. In capite sequentis Π.

Mar. 13, 1781. Multiple. Extremely unequal. The nearest distance 1′ 56″ 45‴, rather full measure. Position 24° 28′ n. following, not extremely accurate. This is the smallest. The next distance 3′ 17″ 19‴, pretty accurate. Position 15° 56′ n. following. A third I did not measure.

43. *θ* Virginis, FL. 51. De quatuor ultima et sequens.

May 14, 1781. Double. Extremely unequal. L. w.; S. d. Distance 1′ 3″ 53‴, inaccurate. Position 24° 55′ n. preceding.

44. *ι* Libræ, FL. 24.

May 24, 1781. Double. Very unequal. L. w.; S. dusky r. Distance 1′ 5″ 10‴, not accurate. Position 22° 31′ f. following.

45. In constellatione Andromedæ.

July 21, 1781. Double. It is a star near *ι* towards *o*. L. r. Distance about 1½ min.

46. *α* Aquilæ, FL. 53.

July 23, 1781. Double. Extremely unequal. L. w.; S. d. Diſtance 2′ 23″ 18‴. Poſition 64° 44′ n. preceding.

47. In conſtellatione Aquilæ, near FL. 35.

July 25, 1781. Double. It is one of the preceding ſtars of a ſmall quartile near *c*, not very near.

48. In conſtellatione Aquilæ, near FL. 35.

July 25, 1781. Double. It is alſo one of the preceding ſtars of a ſmall quartile near *c*, not very near.

49. In conſtellatione Aquilæ.

July 26. Double. The following ſtar of a trapezium near l.

50. In conſtellatione Aquilæ.

July 26, 1781, Double. The following ſtar of a trapezium near l. not near.

51. In monte Mænali Heveliana.

Aug. 5, 1781. Double. It is a ſtar near the middle. The following of two, not very near.

52. In conſtellatione Bootis.

Aug. 17, 1781. Double. It is a ſtar between *e* and *f*. Diſtance above 1′. Unequal.

53. In conſtellatione Bootis.

Aug. 17, 1781. Double. It is a ſtar more ſouth than *i*. Diſtance above 1′.

54. In conſtellatione Serpentarii.

Aug. 21, 1781. Double. It is a ſtar more ſouth than *o*. Diſtance 75″, exact eſtimation.

55. In conſtellatione Caſſiopeæ, FL. 2.

Sept. 6. Double. It is a ſtar near *e*. L. r. Diſt. within 2′½.

56. *θ* Lyræ, FL. ultima.

Sept. 25, 1781. Double. Very unequal. L. w.; S. inclining to r. Diſtance about 1½ min. Poſition —— n. following.

57. In conſtellatione Cygni, FL. 79.

Oct. 1, 1781. Double. It is the fifth ſtar from ρ to υ. Unequal. L. w.; S. pale r. Diſtance 1′ 40″ eſtimation.

58. In conſtellatione Aquarii, FL. 5.

Oct. 5, 1781. Double. It is the moſt ſouth of two in the arrow of Antinous. Diſtance above 1′.

59. In conſtellatione Cygni, near FL. 28.

Oct. 5, 1781. Double. It is a ſtar near *b*. Diſtance 73″, exact eſtimation.

60. In conſtellatione Cygni.

Oct. 8, 1781. Double. It is a ſtar near the ſecond *c*. Conſiderably unequal. L. w.; S. d. Diſtance 88″, exact eſtimation.

61. In conſtellatione Piſcium, near FL. 7.

Oct. 8, 1781. Treble. It is a ſtar preceding *b*. They form a triangle, each ſide of which is about 1′.

62. χ Piſcium, FL. 8. In ventre.

Oct. 8. Double. Diſtance near 2′.

63. In conſtellatione Sagittæ.

Oct. 12, 1781. Double. It is near the ſtar north following ε. Extremely unequal. L. w. inclining to r.; S. d. Diſtance 1′ 30″ 56‴. Poſition 4° 9′ ſ. preceding. A third ſtar in the ſame direction, at a little more than twice the diſtance. A fourth ſtar in view.

64. In conſtellatione Eridani.

Oct. 22, 1781. Double. It is the ſmall ſtar near ν. Diſtance about 1½ min.

65. In capite Monocerotis.

Oct. 22, 1781. Multiple. It is one ſtar with at leaſt 12 around it, all within the field of my teleſcope.

66. α Tauri, FL. 87. Splendida in auſtrina oculo.

Dec. 19, 1781. Double. Extremely unequal. L. r.; S. d. Diſtance 1′ 27″ 45‴. Poſition 52° 58′ n. following. With 460, the apparent diameter of this ſtar, when on the meridian, meaſured 1″ 46‴, a mean of two very compleat obſervations, they agreed to 6‴; with 932, it meaſured 1″ 12‴, alſo a mean of two excellent obſervations; they agreed to 8‴. The apparent diſk was perfectly well defined with both powers.

POSTSCRIPT TO THE CATALOGUE OF DOUBLE STARS.

SINCE my having delivered my paper on the Parallax of the Fixed Stars, in which I refer to the above Catalogue of Double Stars, I have received, by the favour of our Preſident Sir JOSEPH BANKS, the fourth volume of the Acta Academiæ Theodoro Palatinæ, which contains a moſt excellent Memoir of Mr. MAYER's, "De novis in Cœlo ſidereo Phænomenis;" wherein I ſee that the idea of aſcertaining the proper motion of the ſtars by means of ſmall ſtars that are ſituated at no great diſtance from large ones, has induced that gentleman before me to look out for ſuch ſmall ſtars. In the courſe of that undertaking he has diſcovered a good many double ſtars, of which he has given us a pretty large liſt, ſome of them the ſame with thoſe in my catalogue. My view being the annual parallax required ſtars much nearer than thoſe that would do for Mr.

MAYER's

MAYER's purpoſe; therefore I examined the heavens with much higher powers, and looked out chiefly for ſuch as were exceedingly cloſe.

The above catalogue contains 269 double ſtars, 227 of which, to my preſent knowledge, have not been noticed by any perſon. I hope they will prove no inconſiderable addition to the general ſtock, eſpecially as in that number there are a great many which are out of the reach of Mr. MAYER's and other mural quadrant or tranſit inſtruments. It can hardly be expected, that a power of 70 or 80 would be ſufficient to diſcover thoſe curious ſtars that are contained in the firſt claſs of my catalogue; ſo that it is not ſtrange they ſhould have intirely eſcaped Mr. MAYER's notice. We ſee that it is not for want of his looking at thoſe ſtars; for we find he has frequently obſerved ζ Cancri, the ſtar near Procyon, and the ſtar in Monoceros, without perceiving the ſmall ſtars near them, which I have pointed out. Nor is it only in the firſt claſs that his teleſcope wanted power, light, and diſtinctneſs; for the ſmall ſtars that are near β Orionis, β Serpentis, ζ Orionis, *e* Pegaſi, α Lyræ, α Andromedæ, μ Sagittarii, α Aquilæ, η Pegaſi, δ Lyræ, ι Libræ, κ Piſcium, α Tauri, and many more, have eſcaped his diſcovery, though he has given us the places of other more diſtant ſmall ſtars not far from them, and therefore muſt have had them frequently in the field of view of his teleſcope. In ſettling the relative ſituations of very cloſe double ſtars, neither Mr. MAYER's inſtruments, nor his method, were adequate to the purpoſe. It is well known, that whenever we employ time as a meaſure, the reſults cannot be very accurate; becauſe a miſtake of no more than a tenth part of a ſecond in time will produce an error of a whole ſecond and an half in meaſure, ſo that his AR muſt be

be extremely defective. Nor could his micrometer give the declination much better unleſs the teleſcope had bore a power of at leaſt 4 or 500. When the angle of poſition is but ſmall, ſuch as 3, 4, 5, or 6 degrees, and the diſtance of the ſtars not above a few ſeconds, it is evident, that a micrometer muſt be able to meaſure tenths of a ſecond at leaſt to give even a tolerable exactneſs of poſition. On the contrary, the poſition being meaſured with ſuch a micrometer as I have conſtructed for the purpoſe, we may from thence deduce the declination, with great confidence, true to a quarter of a tenth of a ſecond for every ſecond of the diſtance of the ſtars.

Mr. MAYER's account of α Geminorum, for inſtance, gives a difference of $0''{,}7$ of time in AR, of $3''{,}8$ in declination, and of 1 to 6 in magnitude or degree of light of the ſtars. Theſe quantities reduced to my notation, and compared with my meaſures of the ſame ſtar, give

	Mr. MAYER's	Mine
Diſtance	$9''{,}635$ from center to center	$5''{,}156$ diameters included.
Poſition	23° 14′ n. preceding	32° 47′ n. preceding.
Magnitude	extremely unequal	A little unequal.

To account for this difference I aſcribe Mr. MAYER's error in diſtance to his method of meaſuring by time. The error of poſition follows always from an obſervation of the declination taken with the common micrometer, when it is deduced from an erroneous AR. In my meaſures the diſtance and poſition are independent of each other, which I look upon as no ſmall advantage of my croſs-hair micrometer. The error in the magnitudes of the ſtars I aſcribe to the want of power in Mr. MAYER's teleſcope, which did not ſeparate the ſtars far enough for him to judge accurately of their ſize, otherwiſe he would ſoon have found, that inſtead of five there is hardly ſo much as one

one ſingle degree of difference in their magnitudes. See fig. 6. for a repreſentation of thoſe ſtars with my power of 460.

I do not mean to depreciate Mr. MAYER's method, the excellence of which is well known; and with ſome ſtars of my third, all thoſe of the fourth, fifth, and ſixth claſſes, as well as with thoſe ſtill farther diſtant, to which he has applied it with admirable ſkill, and "magno labore, multiſque "nocturnis vigillis" (as he very juſtly expreſſes himſelf) a better can hardly be wiſhed for; but with ſtars of the ſecond claſs which generally differ no more than one, two or three-tenths of a ſecond of time in $\mathcal{R}$, and can never differ more than four tenths, the inſufficiency of meaſuring by time is obvious. In regard to the declination, it is alſo no leſs evident, that it is much more accurate to take an angle, which may be had true to 2 or 3″ at moſt, than to meaſure its tangent, which in ſtars of the ſecond claſs is generally no more than 2, 3, or 4″ of a degree, and can never exceed five. I do not ſo much as mention the ſtars of the firſt claſs: they muſt certainly, as to ſenſe, paſs the meridian at the ſame inſtant of time. Their diſtance has even eluded the attacks of my ſmalleſt ſilk-thread micrometer armed with an excellent power of 460; but I ſhall ſoon apply my laſt new inſtrument to them *, not without hopes of ſucceſs. Now, though I have hitherto not been able to expreſs the diſtance of the ſtars of the firſt claſs, otherwiſe than by the proportion it bears to their apparent diameters, I think it a very great point gained, that one of my inſtruments at leaſt (*viz.* the croſs-hair micrometer) has laid hold of them: for their angle of poſition, I think, is within a very ſmall quantity as well determined as it is in thoſe of the ſecond claſs. This ſimple but moſt uſeful inſtrument can, by actual meaſure,

* For a deſcription of which ſee p. 163.

diſcover

diſcover beyond a doubt a motion in two ſtars that are very cloſe together, though it ſhould amount to no more than a tenth part of a ſecond of a degree, provided that motion be in ſuch a direction that the effect of it be thrown upon the angle of poſition; wherein, with ſome of the ſtars of the firſt claſs, it would occaſion an alteration of 10, 20, 30, or more degrees.

I have marked all thoſe ſtars in my catalogue which have been obſerved by Mr. MAYER and other aſtronomers with an aſteriſk (*) affixed to the number that they may be known; thoſe with the mark of a dagger (†) have been obſerved by different aſtronomers before Mr. MAYER. Among the ſtars which are not marked, will be found ſeveral that have been obſerved by Mr. MAYER; but, on comparing them together, it will be ſeen, that they are obſervations of different ſmall ſtars; for inſtance, Mr. MAYER (Act. Acad. vol. IV. p. 296.) obſerved a ſmall ſtar near Rigel at the diſtance of $1' 8'',5$ $A\!R$ in time, and $2'\ 55'',2$ in difference of declination north preceding Rigel. In my ſecond claſs (the 34th ſtar) we alſo find Rigel; but the ſmall ſtar I have obſerved is one which has not been ſeen by Mr. MAYER, and is at a diſtance of no more than $6'\ 27''$. Poſition 68° 12′ ſouth preceding; and ſo on with other ſtars.

I have uſed the expreſſion *double-ſtar* in a few inſtances of the ſixth claſs in rather an extended ſignification: the example of FLAMSTEAD, however, will ſufficiently authorize my application of the term. I preferred that expreſſion to any other, ſuch as Comes, Companion, or Satellite; becauſe, in my opinion, it is much too ſoon to form any theories of ſmall ſtars revolving round large ones, and therefore I thought it adviſeable carefully to avoid any expreſſion that might convey that idea. I am

very well perſuaded, FLAMSTEAD, who firſt uſed the word Comes, meant it only in a figurative ſenſe.

I ſhall not fail to take the firſt opportunity of looking out for thoſe of Mr. MAYER's double-ſtars which I have not in my catalogue, amounting to 31; and alſo for one I find mentioned in *La Connoiſſance des Temps* for 1783, diſcovered by Mr. MESSIER.

XIII. *Description of a Lamp-Micrometer, and the Method of using it.* By Mr. William Herſchel, *F. R. S.*

Read January 31, 1782.

THE great difficulty of meaſuring very ſmall angles, ſuch as hardly amount to a few ſeconds, is well known to aſtronomers. Since I have been engaged in obſervations on double ſtars, I have had ſo much occaſion for micrometers that would meaſure exceeding ſmall diſtances exactly, that I have continually been endeavouring to improve theſe inſtruments.

The natural imperfections of the parallel wire micrometer in taking the diſtance of very cloſe double ſtars are the following. When two ſtars are taken between the parallels, the diameters muſt be included. I have in vain attempted to find lines ſufficiently thin to extend them acroſs the centers of the ſtars ſo that their thickneſs might be neglected. The ſingle threads of the ſilk-worm, with ſuch lenſes as I uſe, are ſo much magnified that their diameter is more than that of many of the ſtars. Beſides, if they were much leſs than they are, the power of deflection of light would make the attempt to meaſure the diſtance of the centers this way fruitleſs: for I have always found the light of the ſtars to play upon thoſe lines and ſeparate their apparent diameters into two parts. Now ſince the ſpurious diameters of the ſtars thus included, to my certain knowledge, are continually changing according to the ſtate of the air, and the length of time we look at them, we are, in

ſome reſpect, leſt at an uncertainty, and our meaſures taken at different times, and with different degrees of attention, will vary on that account. Nor can we come at the true diſtance of the centers of any two ſtars, one from another, unleſs we could tell what to allow for the ſemi-diameters of the ſtars themſelves; for different ſtars have different apparent diameters, which, with a power of 227, may differ from each other (as I have experienced) as far as two ſeconds.

The next imperfection is that which ariſes from a deflection of light upon the wires when they approach very near to each other; for if this be owing to a power of repulſion lodged at the ſurface, it is eaſy to underſtand, that ſuch powers muſt interfere with each other, and give the meaſures larger in proportion than they would have been if the repulſive power of one wire had not been oppoſed by a contrary power of the other wire.

Another very conſiderable imperfection of theſe micrometers is a continual uncertainty of the real zero. I have found, that the leaſt alteration in the ſituation and quantity of light will affect the zero, and that a change in the poſition of the wires, when the light and other circumſtances remain unaltered, will alſo produce a difference. To obviate this difficulty, whenever I took a meaſure that required the utmoſt accuracy, my zero was always taken immediately after, while the apparatus remained in the ſame ſituation it was in when the meaſure was taken; but this enhances the difficulty becauſe it introduces an additional obſervation.

The next imperfection, which is none of the ſmalleſt, is that every micrometer that has hitherto been in uſe requires either a ſcrew or a divided bar and pinion to meaſure the diſtance of the wires or divided image. Thoſe who are acquainted

quainted with works of this kind are but too sensible how difficult it is to have screws that shall be perfectly equal in every thread or revolution of each thread; or pinions and bars that shall be so evenly divided as perfectly to be depended upon in every leaf and tooth to perhaps the two, three, or four thousandth part of an inch; and yet, on account of the small scale of those micrometers, these quantities are of the greatest consequence; an error of a single thousandth part inducing in most instruments a mistake of several seconds.

The last and greatest imperfection of all is, that these wire micrometers require a pretty strong light in the field of view; and when I had double stars to measure, one of which was very obscure, I was obliged to be content with less light than is necessary to make the wires perfectly distinct; and several stars on this account could not be measured at all, though otherwise not too close for the micrometer.

The instrument I am going to describe, which I call a Lamp-Micrometer, is free from all these defects, and has, moreover, to recommend it, the advantage of a very enlarged scale. The construction of it is as follows.

ABGCFE (fig. 1.) is a stand nine feet high, upon which a semi-circular board *qhogp* is moveable upwards or downwards, in the manner of some fire-screens, as occasion may require, and is held in its situation by a peg *p* put into any one of the holes of the upright piece AB. This board is a segment of a circle of fourteen inches radius, and is about three inches broader than a semi-circle, to give room for the handles rD, rP, to work. The use of this board is to carry an arm L, thirty inches long, which is made to move upon a pivot at the center of the circle, by means of a string, which passes in a groove upon the edge of the semi-circle *pghq*; the string is fastened

to

to a hook at *o* (not expressed in the figure being at the back of the arm L), and passing along the groove from *o* to *q* is turned over a pulley at *q*, and goes down to a small barrel *r*, within the plane of the circular board, where a double-jointed handle P commands its motion. By this contrivance we see the arm L may be lifted up to any altitude from the horizontal position to the perpendicular, or be suffered to descend by its own weight below the horizontal to the reverse perpendicular situation. The weight of the handle P is sufficient to keep the arm in any given position; but if the motion should be too easy, a friction spring applied to the barrel will moderate it at pleasure.

In front of the arm L a small slider, about three inches long, is moveable in a rabbet from the end L towards the center backwards and forwards. A string is fastened to the left side of the little slider, and goes towards L, where it passes round a pulley at *m*, and returns under the arm from *m*, *n*, towards the center, where it is led in a groove on the edge of the arm, which is of a circular form, upwards to a barrel (raised above the plane of the circular board) at *r*, to which the handle D is fastened. A second string is fastened to the slider, at the right side, and goes towards the center, where it passes over a pulley *s*, and the weight *w*, which is suspended by the end of this string, returns the slider towards the center when a contrary turn of the handle permits it to act.

a and *b* are two small lamps, two inches high, 1¼ in breadth by 1¼ in depth. The sides, back, and top, are made so as to permit no light to be seen, and the front consists of a thin brass sliding door. The flame in the lamp *a* is placed three-tenths of an inch from the left side, three-tenths from the front, and half an inch from the bottom. In the lamp *b* it is placed at

the

the same height and distance measuring from the right side. The wick of the flame consists only of a single very thin lamp-cotton thread; for the smallest flame being sufficient it is easier to keep it burning in so confined a place. In the top of each lamp must be a little slit, lengthways, and also a small opening in one side near the upper part, to permit air enough to circulate to feed the flame. To prevent every reflection of light, the side opening of the lamp *a* should be to the right, and that of the lamp *b* to the left. In the sliding door of each lamp is made a small hole with the point of a very fine needle just opposite the place where the wicks are burning, so that when the sliders are shut down, and every thing dark, nothing shall be seen but two fine lucid points of the size of two stars of the third or fourth magnitude. The lamp *a* is placed so that its lucid point may be in the center of the circular board where it remains fixed. The lamp *b* is hung to the little slider which moves in the rabbet of the arm, so that its lucid point, in a horizontal position of the arm, may be on a level with the lucid point in the center. The moveable lamp is suspended upon a piece of brass fastened to the slider by a pin exactly behind the flame upon which it moves as a pivot. The lamp is balanced at the bottom by a leaden weight, so as always to remain upright, when the arm is either lifted above, or depressed below, the horizontal position. The double-jointed handles *r*D, *e*P, consist of light deal rods, ten feet long, and the lowest of them may have divisions, marked upon it near the end P, expressing exactly the distance from the central lucid point in feet, inches, and tenths.

From this construction we see, that a person at a distance of ten feet may govern the two lucid points, so as to bring them into any required position south or north preceding or following,

from 0 to 90° by using the handle F, and also to any distance from six-tenths of an inch to five or six and twenty inches by means of the handle D. If any reflection or appearance of light should be left from the top or sides of the lamps, a temporary screen, consisting of a long piece of paste-board, or a wire frame covered with black cloth, of the length of the whole arm and of any required breadth, with a slit of half an inch broad in the middle, may be affixed to the arm by four bent wires projecting an inch or two before the lamps, situated so that the moveable lucid point may pass along the opening left for that purpose.

Fig. 2. represents part of the arm L, half the real size; S the slider; *m* the pulley, over which the cord *xlyz* is returned towards the center; *v* the other cord going to the pulley *n* of fig. 1. R the brass piece moveable upon the pin *c*, to keep the lamp upright. At R is a wire rivetted to the brass piece, upon which is held the lamp by a nut and screw. Fig. 3. 4. represent the lamps *a*, *b*, with the sliding doors open, to shew the situation of the wicks. W is the leaden weight with a hole *d* in it, through which the wire R of fig. 2. is to be passed when the lamp is to be fastened to the slider S. Fig. 5. represents the lamp *a* with the sliding door shut; *l* the lucid point; and *ik* the openings at the top, and *s* at the sides for the admission of air.

Every ingenious artist will soon perceive that the motions of this micrometer are capable of great improvement by the application of wheels and pinions, and other well known mechanical resources; but, as the principal object is only to be able to adjust the two lucid points to the required position and distance, and to keep them there for a few minutes, while the observer

obſerver goes to meaſure their diſtance, it will not be neceſſary to ſay more upon the ſubject.

I am now to ſhew the application of this inſtrument. It is well known to opticians and others, who have been in the habit of uſing optical inſtruments, that we can with one eye look into a microſcope or teleſcope, and ſee an object much magnified, while the naked eye may ſee a ſcale upon which the magnified picture is thrown. In this manner I have generally determined the power of my teleſcopes; and any one who has acquired a facility of taking ſuch obſervations will very ſeldom miſtake ſo much as one in fifty in determining the power of an inſtrument, and that degree of exactneſs is fully ſufficient for the purpoſe.

The Newtonian form is admirably adapted to the uſe of this micrometer; for the obſerver ſtands always erect, and looks in a horizontal direction, notwithſtanding the teleſcope ſhould be elevated to the zenith. Beſides, his face being turned away from the object to which his teleſcope is directed, this micrometer may be placed very conveniently without cauſing the leaſt obſtruction to the view: therefore, when I uſe this inſtrument I put it at ten feet diſtance from the left eye, in a line perpendicular to the tube of the teleſcope, and raiſe the moveable board to ſuch a height that the lucid point of the central lamp may be upon a level with the eye. The handles, lifted up, are paſſed through two loops faſtened to the tube, juſt by the obſerver, ſo as to be ready for his uſe. I ſhould obſerve, that the end of the tube is cut away ſo as to leave the left eye intirely free to ſee the whole micrometer.

Having now directed the teleſcope to a double ſtar, I view it with the right eye, and at the ſame time with the left ſee it pro-

jected upon the micrometer: then, by the handle P, which commands the poſition of the arm, I raiſe or depreſs it ſo as to bring the two lucid points to a ſimilar ſituation with the two ſtars; and, by the handle D, I approach or remove the moveable lucid point to the ſame diſtance of the two ſtars, ſo that the two lucid points may be exactly covered by, or coincide with the ſtars. A little practice in this buſineſs ſoon makes it eaſy, eſpecially to one who has already been uſed to look with both eyes open.

What remains to be done is very ſimple. With a proper rule, divided into inches and fortieth parts, I take the diſtance of the lucid points, which may be done to the greateſt nicety, becauſe, as I obſerved before, the little holes are made with the point of a very fine needle. The meaſure thus obtained is the tangent of the magnified angle under which the ſtars are ſeen to a radius of ten feet; therefore, the angle being found and divided by the power of the teleſcope gives the real angular diſtance of the centers of a double ſtar.

For inſtance, September 25, 1781, I meaſured α Herculis with this inſtrument. Having cauſed the two lucid points to coincide exactly with the ſtars center upon center, I found the radius or diſtance of the central lamp from the eye 10 feet 4,15 inches; the tangent or diſtance of the two lucid points 50,6 fortieth parts of an inch; this gives the magnified angle $35'$, and dividing by the power 460, which I uſed, we obtain $4'' 34'''$ for the diſtance of the centers of the two ſtars. The ſcale of the micrometer at this very convenient diſtance, with the power of 460 (which my teleſcope bears ſo well upon the fixed ſtars that for near a twelve-month paſt I have hardly uſed any other) is above a quarter of an inch to a ſecond; and by putting on my power of 932, which in very fine evenings is

extremely distinct, I obtain a scale of more than half an inch to a second, without increasing the distance of the micrometer; whereas the most perfect of my former micrometers, with the same instrument, had a scale of less than the two thousandth part of an inch to a second.

The measures of this micrometer are not confined to double stars only, but may be applied to any other objects that require the utmost accuracy, such as the diameters of the planets or their satellites, the mountains of the moon, the diameters of the fixed stars, &c.

For instance, October 22, 1781, I measured the apparent diameter of α Lyræ; and judging it of the greatest importance to increase my scale as much as convenient, I placed the micrometer at the greatest convenient distance, and (with some trouble, for want of longer handles, which might easily be added) took the diameter of this star by removing the two lucid points to such a distance as just to include the apparent diameter. When I measured my radius it was found to be twenty-two feet six inches. The distance of the two lucid points was *about* three inches; for I will not pretend to *extreme* nicety in this observation, on account of the very great power I used, which was 6450. From these measures we have the magnified angle 38′ 10″: this divided by the power gives 0″,355 for the apparent diameter of α Lyræ. The scale of the micrometer, on this occasion, was no less than 8,443 inches to a second, as will be found by multiplying the natural tangent of a second with the power and radius in inches.

November 28, 1781, I measured the diameter of the new star; but the air was not very favourable, for this singular star was not so distinct with 227 that evening as it generally is

with 460: therefore, without laying much ſtreſs upon the exactneſs of the obſervation, I ſhall only report it to exemplify the uſe of the micrometer. My radius was 35 feet 11 inches. The diameter of the ſtar, by the diſtance of the lucid points, was 2,4 inches, and the power I uſed 227: hence the magnified angle is found 19′, and the real diameter of the ſtar 5″,022. The ſcale of this meaſure ,474 milleſimals of an inch, or almoſt half an inch to a ſecond.

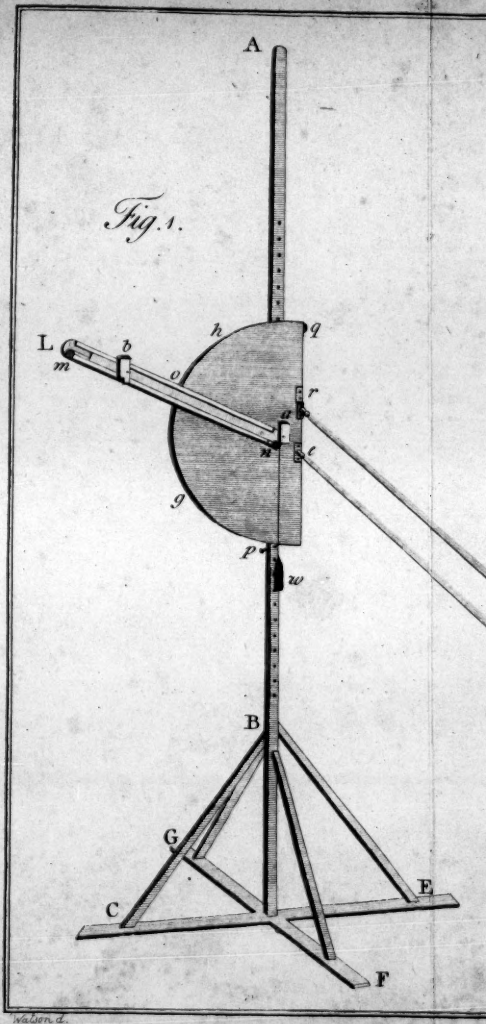


Fig. 1.
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Fig. 4.

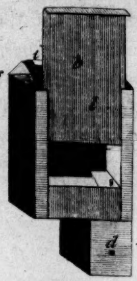


Fig. 3.

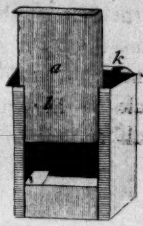


Fig. 5.

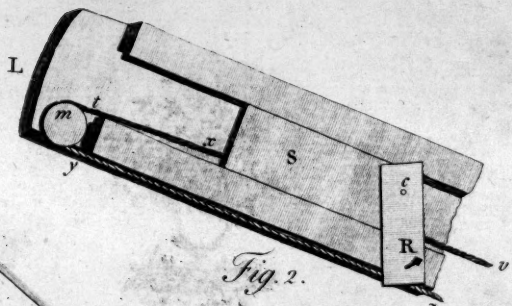
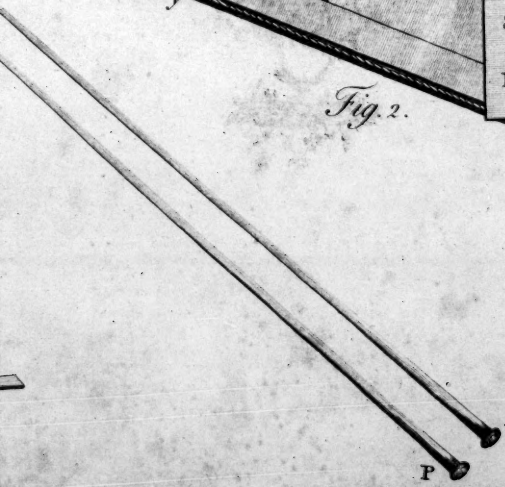


Fig. 2.



Basire d.

XIV. *A Paper to obviate ſome Doubts concerning the great Magnifying Powers uſed. By Mr.* Herſchel, *F. R. S.*

TO SIR JOSEPH BANKS, BART. P. R. S.

SIR,

I HAVE the honour of laying before you the reſult of a ſet of meaſures I have taken in order to aſcertain once more the powers of my Newtonian ſeven-feet reflector. The method I have formerly uſed, and which I ſtill prefer to that which I have now been obliged to practiſe, requires very fine weather and a ſtrong ſun-ſhiny day; but my impatience to anſwer the requeſts of Sir JOSEPH BANKS would not permit me to wait for ſo precarious an opportunity at this ſeaſon of the year. The difference in all the powers, as far as 2010, will be found to be in favour of thoſe I have mentioned; and, I believe, a much greater concurrence could not well be expected, where different

different methods of aſcertaining them are uſed. The variation in the two higheſt powers is more conſiderable than I was aware of; but ſtill may eaſily be ſhewn to be a neceſſary conſequence of the difference in the methods. However, if upon comparing together the methods it ſhould be thought, that the power 5786 is nearer the truth than 6450, I ſhall readily join to correct that number. The manner in which I have now determined the powers is as follows: I took one of the eye lenſes which magnifies leaſt, and meaſured its ſolar focus by the ſun's rays as exactly as I could five times, which proved to be 1.01, 1.04, 1.09, 1.01, 1.05, in half-inch meaſure, a mean of which is 1.04. The ſidereal focus of my ſeven-feet ſpeculum is 170.4 in the ſame meaſure. Thence, dividing 170.4 by 1.04 we find that the teleſcope will magnify 163.8 times when that lens is uſed. This power being found, I applied the ſame lens as a ſingle microſcope to view with it a certain object, which was a drawn braſs wire faſtened ſo as not to turn upon its axis or change its poſition; for theſe wires are ſeldom perfectly round, or of an even ſize, and it is therefore neceſſary to uſe this precaution to prevent errors: then, with a fine pair of compaſſes, I took four independent meaſures of the image of the braſs wire, which was thrown upon a ſheet of paper exactly 8¼ inches from the lens, the eye being always as cloſe to the lens as poſſible. I viewed the ſame wire, exactly in the ſame manner, with every one of the lenſes, and meaſured the pictures upon the paper. When I came to the higher powers the wire was exchanged for another 4.37 times thinner than the former, as determined by comparing the proportion of their images 54 to 235¼, taken by the ſame lens.

When the images of theſe wires are obtained, the power of the teleſcope, with every one of the lenſes, becomes known by

by one plain analogy: *viz.* as the image of the wire by the firſt lens (77¼) is to the power it gives to the teleſcope (163.8), ſo is the image of the wire by the ſecond lens (119) to the power it will give to the ſame teleſcope (250.7). The particulars of all the meaſures are as follows:

Powers as they have been called in my papers.	Images of a wire thrown upon a paper in hundredths of half inches.				A mean of the four meaſures.	Powers as they come out by this method.
146	77	78	78	78	77¼	163.86 = $\frac{170\frac{1}{4}}{1.04}$
227	119	119	119	119	119	250.7
278	143	143	144	143	143¼	301.8
460	236	236	235	236	235¾	496.7
	Smaller wire.					
	53	54	55	54	54	
754	83	85	84	85	84¼	775.1
932	107	107	107	108	107¼	986.7
1159	128	128	129	128	128¼	1179.9
1536	An excellent lens, loſt about eight months ago.					
2010	236	236	238	336	236¼	2175.8
3168	281	283	281	280	281¼	2585.5
6450	635	625	630	626	629	5786.8

I beg leave, Sir, now to give a ſhort deſcription of the method I have formerly uſed to determine theſe powers. In the year 1776 I erected a mark of white paper, exactly half an inch in diameter, which I viewed with my teleſcope at the greateſt convenient diſtance with one of the leaſt magnifiers. An aſſiſtant was placed at rectangles in a field, at the ſame diſtance

from my eye as the object from the great ſpeculum of the teleſcope. Upon a pole erected there I viewed the magnified image of the half inch, and the aſſiſtant marked it by my direction; this being meaſured gave the power of the inſtrument at once. The power thus obtained was corrected by theory, to reduce it to what it would be upon infinitely diſtant objects. The powers of the reſt of the lenſes I deduced from this by a *Camera-eye-piece*, which I made for that purpoſe. ABCD (fig. 1.) repreſents a perpendicular ſection of it. The end A ſcrews into the teleſcope. Upon the end B may be ſcrewed any of the common ſingle-lens eye-pieces. *lmn* is a ſmall oval plane ſpeculum, adjuſted to an angle of 45° by three ſcrews, two whereof appear at *op*. When the obſerver looks in at B, he may ſee the object projected upon a ſheet of paper on a table placed under the Camera-piece, and meaſure its picture *a*, *b*, as in fig. 2. The power of one lens therefore being known, that of the reſt was alſo found by comparing the meaſures of the projected images.

It may not be amiſs to mention ſome of the advantages and inconveniencies attending each of theſe methods. When we take the focus of an eye-lens, which the firſt method requires, we are liable to a pretty conſiderable uncertainty, and in very ſmall lenſes it is not to be done at all. Moreover, in calculating the power by that focus no account is made of the aberration which takes place in all ſpecula and lenſes, and increaſes the image, ſo that we rather find out how much the teleſcope *ſhould* magnify than how much it really *does* magnify; but in determining the power by an experiment we avoid theſe difficulties.

On the other hand, when the power is very great, the latter method becomes inconvenient, both on account of want of light

light in the object, and a very conſiderable aberration which takes place, and makes the picture too indiſtinct to be very accurate in the meaſure, and of courſe larger than it ought to be; and this will account for the exceſs in the meaſures of my two largeſt powers. However, when I employed 6450 upon the diameter of α Lyræ, I incline to think the method I had uſed when I determined that power, ought to be preferred, becauſe my Lamp-micrometer gives the meaſure of an object as it appears in the teleſcope, and therefore this aberration is included, and ſhould be taken into conſideration.

To prevent any miſtakes, I wiſh to mention again, that I have all along proceeded *experimentally* in the uſe of my powers, and that I do not mean to ſay I have uſed 6450 (or 5786) upon the planets, or even upon double ſtars; every power I have mentioned is to be underſtood as having been uſed juſt as it is related; but farther inferences ought not as yet to be drawn. For inſtance, my obſervations on ϵ Bootis mention that I have viewed that ſtar with 2010 (or as in the above table with 2175) extremely diſtinct; but upon ſeveral other celeſtial objects I have found this power of no ſervice. Many plauſible ſuggeſtions have already occurred to account for theſe appearances; but I wait till farther experiments ſhall have furniſhed me with more materials to reaſon upon. The uſe of high powers is a new and untrodden path, and in this attempt variety of new phænomena may be expected, therefore I wiſh not to be in a haſte to make general concluſions. I ſhall not fail to purſue this ſubject, and hope ſoon to be able to attack the celeſtial bodies with a ſtill ſtronger armament, which is now preparing.

It remains now only for me to make the moſt ſincere acknowledgement for the favours you have ſhewn to me, and to ſay that I ſhall ever remain, with equal reſpect and gratitude,

SIR, your moſt obedient, &c.

P. S. Dr. WATSON junior has done me the favour ſeparately to examine and meaſure the powers of my teleſcope; and placing the greateſt confidence in his accuracy, I rely on his meaſures at leaſt as much as my own.

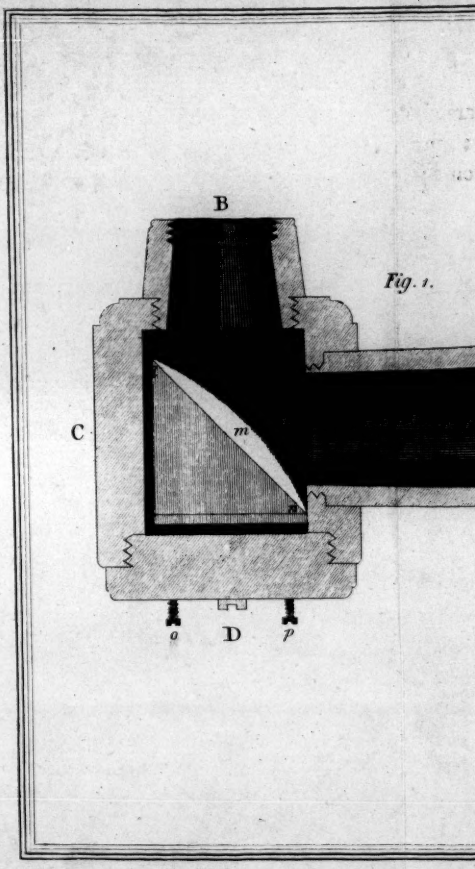


Fig. 1.

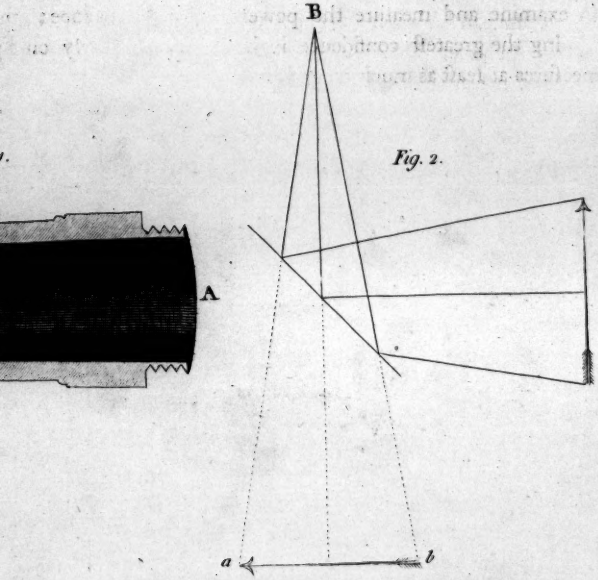


Fig. 2.

XIV. *Continuation of the Experiments and Observations on the Specific Gravities and Attractive Powers of various Saline Substances.* By Richard Kirwan, *Esq. F. R. S.*

Read April 11, 1782.

BEFORE I enter into a detail of the new experiments I have made in the prosecution of this subject, I must beg leave to rectify some mistakes I have fallen into in my last paper.

1. In computing the quantity of acid taken up by 10,5 gr. of mild vegetable fixed alkali, I made no allowance for the small quantity of earth it contains, *viz.* 0,7035 of a grain; but in large quantities of alkali, this proportion is considerable, and occasioned a small but sensible error in my subsequent calculations of the proportion of ingredients in neutral salts, the quantity of alkali being, by that fraction, less than I supposed it in 10,5 gr. This correction being made, it will be found, that 100 gr. of perfectly dry vegetable fixed alkali (abstracted from the quantity of earth) generally contain 22,457 gr. of fixed air instead of 21, as I before determined; yet the former determination is right, where the earth is not separated, yet may well be supposed to exist, as in the alkali of pearl-ash, purified by three repeated calcinations and solutions. Hence also 100 gr. of such alkali, free from earth, water, and fixed air, take up 46,77 gr. of the mineral acids, that is, of the mere

acid part; and 100 gr. of common mild vegetable alkali take up about 36,23 of real acid.

100 gr. of perfectly dry tartar vitriolate contain 30,21 of real acid, 64,61 of fixed alkali, and 5,18 of water. Cryſtallized tartar vitriolate loſes only 1 *per cent.* of water in a heat in which its acid alſo is not ſeparated in any degree, and therefore contains 6,18 of water.

100 gr. of nitre, perfectly dried, contain 30,86 of acid, 66 of alkali, and 3,14 of water; but in cryſtallized nitre the proportion of water is ſomewhat greater; for 100 gr. of theſe cryſtals, being expoſed to a heat of 180° for two hours, loſt 3 gr. of their weight, without exhaling any acid ſmell; but when expoſed to a heat of 200°, the ſmell of the nitrous acid is diſtinctly perceived. Hence 100 gr. of cryſtallized nitre contain 29,89 of mere acid, 63,97 of alkali, and 6,14 of water.

100 gr. of digeſtive ſalt perfectly dry contain 29,68 of marine acid, 63,47 of alkali, and 6,85 of water. 100 gr. of cryſtallized digeſtive ſalt loſe but 1 gr. of their weight before the ſmell of the marine acid is perceived; and hence they contain 7,85 gr. of water.

But the miſtake which coſt me moſt time and pains to correct was that which I fell into when I imagined, that the mixtures of oil of vitriol and water, and ſpirit of nitre and water, had attained their maximum of denſity when they had cooled to the temperature of the atmoſphere, which at the time I made my experiments ſtood between 50 and 60° of FAHRENHEIT. The former I had even ſuffered to ſtand ſix hours, which was much longer than was neceſſary for its cooling; but when the acid was ſo much diluted as to cauſe little or no heat, I allowed it to ſtand but for a very little time before I examined its denſity: yet ſeveral months after I found many of theſe mixtures

mixtures much denſer than when I firſt examined them, and that at leaſt twelve hours reſt was requiſite before concentrated oil of vitriol, to which even twice its weight of water is added, attains its utmoſt denſity, and ſtill more when a leſſer proportion of water is uſed: thus, when I made the mixture of 2519,75 gr. of oil of vitriol, whoſe ſpecific gravity was 1,819, with 180 of water, I found its denſity ſix hours after 1,771; but after twenty-four hours it was 1,798: and hence, according to the reaſoning in the former paper, the accrued denſity was at leaſt ,064 inſtead of ,045 as I had formerly found it. But by uſing oil of vitriol ſtill more concentrated, whoſe ſpecific gravity was 1,8846, I was enabled, by a ſimilar train of reaſoning, to make a ſtill nearer approximation, and found that the accrued denſity of oil of vitriol, whoſe ſpecific gravity is 1,819, amounts to 0,104; and conſequently its mathematical ſpecific gravity is 1,715. 6,5 gr. of this oil of vitriol contained, as I before found, 3,55 of mere acid, and the remainder water, then the weight of an equal bulk of water is 3,79 gr.; and ſubtracting from this the weight of the water that enters into the compoſition of the oil of vitriol, it will be found, that the weight of a bulk of water, equal to the acid part, is 0,84, and conſequently the ſpecific gravity of the pure and mere acid part is 4,226. Upon this ground, and conſtantly allowing the mixtures to reſt at leaſt twelve hours (until the oil of vitriol was diluted with four times its weight of water, and then often only ſix hours) before their denſity was examined, I conſtructed the table hereto annexed; the temperature of the room I conſtantly kept between 50 and 60°.

Oil

Oil of vitriol.	Acid.	Water.	Accrued denſity.	Mathemat. ſp. gravity.	
Grains.					
1000	— —	387,95	,107	1,877	1,846
1100	— —	487,95	,104	1,738	1,844
1200	— —	587,95	,105	1,637	1,742
1300	— —	687,95	,144	1,561	1,705
1400	— —	787,95	,144	1,500	1,644
1500	— —	887,95	,137	1,452	1,589
1600	— —	987,95	,137	1,412	1,539
1700	— —	1087,95	,130	1,379	1,509
1800	— —	1187,95	,124	1,350	1,474
1900	— —	1287,95	,116	1,326	1,442
2000	— —	1387,95	,116	1,304	1,420
2100	— —	1487,95	,112	1,286	1,398
2200	— —	1587,95	,112	1,269	1,381
2300	— —	1687,95	,108	1,254	1,362
2400	— —	1787,95	,104	1,241	1,345
2500	— —	1887,95	,104	1,229	1,333
2600	— —	1987,95	,101	1,219	1,320
2700	— —	2087,95	,096	1,209	1,307
2800	— —	2187,95	,091	1,200	1,291
2900	— —	2287,95	,090	1,192	1,282
3000	— —	2387,95	,090	1,184	1,274
3100	612,05	2487,95	,090	1,177	1,267
3200	— —	2587,95	,090	1,170	1,260
3300	— —	2687,95	,089	1,164	1,253
3400	— —	2787,95	,084	1,159	1,243
3500	— —	2887,95	,083	1,150	1,233
3600	— —	2987,95	,073	1,149	1,222
3700	— —	3087,95	,073	1,144	1,217
3800	— —	3187,95	,071	1,140	1,211
3900	— —	3287,95	,071	1,136	1,208
4000	— —	3387,95	,071	1,132	1,204
4100	— —	3487,95	,070	2,128	1,198
4200	— —	3587,95	,070	1,125	1,195
4300	— —	3687,95	,070	1,121	1,191
4400	— —	3787,95	,070	1,118	1,188
4500	— —	3887,95	,070	1,115	1,185
4600	— —	3987 95	,070	1,113	1,183
4700	— —	4087,95	,070	1,110	1,180
4800	— —	4187,95	,070	1,107	1,177
4900	— —	4287,95	,070	1,105	1,175
5000	— —	4387,95	,070	1,103	1,172
5100	— —	4487,95	,069	1,100	1,169

Oil

Oil of vitriol.	Acid.	Water.	Accrued denſity	Mathemat. ſp. gravity.	
Grains.					
5200	– –	4587,95	,069	1,098	1,167
5300	– –	4687,95	,069	1,096	1,165
5400	– –	4787,95	,069	1,094	1,163
5500	– –	4887,95	,068	1,092	1,160
5600	– –	4987,95	,067	1,091	1,158
5700	– –	5087,95	,067	1,089	1,156
5800	– –	5187,95	,067	1,087	1,154
5900	– –	5287,95	,065	1,086	1,151
6000	– –	5387,95	,064	1,084	1,148
6100	612,05	5487,95	,064	1,082	1,146
6200	– –	5587,95	,063	1,081	1,144
6300	– –	5687,95	,062	1,080	1,142
6400	– –	5787,95	,062	1,078	1,140
6500	– –	5887,95	,061	1,077	1,138
6600	– –	5987,95	,060	1,076	1,136
6700	– –	6087,95	,060	1,074	1,134
6800	– –	6187,95	,060	1,072	1,132
6900	– –	6287,95	,060	1,070	1,130
7000	– –	6387,95	,059	1,069	1,128

With regard to the nitrous acid, I found alſo I had been a little too precipitate as to the time of examining its denſity after it had been mixed with water. Hence, making uſe of ſome whoſe ſpecific gravity was 1,474, I allowed the mixtures to reſt twelve hours, until it was diluted with twice its weight of water, and the ſubſequent mixtures ſix hours at leaſt; by the former proceſs of reaſoning, I found the ſpecific gravity of the mere nitrous acid to be 5,530.

Spirit

Spirit of nitre.	Acid.	Water.	Accrued denſity.	Mathemat. ſp. gravity.	Phyſical ſp. gravity.
900	– –	507	– –	1,557	1,557
1000	– –	607	– –	1,474	1,474
1100	– –	707	,035	1,413	1,448
1200	– –	807	,056	1,367	1,423
1300	– –	907	,065	1,329	1,394
1400	– –	1007	,065	1,298	1,363
1500	– –	1107	,077	1,273	1,350
1600	– –	1207	,082	1,251	1,333
1700	– –	1307	,082	1,233	1,315
1800	– –	1407	,083	1,217	1,300
1900	– –	1507	,083	1,204	1,287
2000	– –	1607	,096	1,191	1,269
2100	– –	1707	,088	1,181	1,254
2200	– –	1807	,071	1,176	1,247
2300	– –	1907	,068	1,162	1,230
2400	– –	2007	,068	1,154	1,222
2500	– –	2107	,067	1,147	1,214
2600	– –	2207	,065	1,141	1,206
2700	– –	2307	,063	1,135	1,198
2800	– –	2407	,061	1,129	1,190
2900	– –	2507	,058	1,124	1,182
3000	– –	2607	,055	1,120	1,175
3100	393	2707	,054	1,116	1,170
3200	– –	2807	,054	1,111	1,165
3300	– –	2907	,053	1,108	1,161
3400	– –	3007	,052	1,104	1,156
3500	– –	3107	,050	1,101	1,151
3600	– –	3207	,048	1,098	1,146
3700	– –	3307	,047	1,095	1,142
3800	– –	3407	,045	1,092 or 3	1,137
3900	– –	3507	,043	1,089	1,132
4000	– –	3607	,040	1,087	1,127
4100	– –	3707	,037	1,085	1,122
4200	– –	3807	,035	1,083	1,118
4300	– –	3907	,034	1,080	1,114
4400	– –	4007	,032	1,078	1,110
4500	– –	4107	,029	1,077	1,106
4600	– –	4207	,027	1,075	1,102
4700	– –	4307	,025	1,073	1,098
4800	– –	4407	,022	1,072	1,094
4900	– –	4507	,020	1,070	1,090
5000	– –	4607	,018	1,068	1,086
5100	– –	4707	,015	1,067	1,082
5200	– –	4807	,012	1,066	1,078
5300	– –	4907	,008	1,066	1,074

The foregoing experiments were made at the temperature of between 50 and 60° of FAHRENHEIT; but as it may be suspected, that the density of the above acids is considerably altered at degrees of temperature considerably different, I endeavoured to find the quantity of this alteration, and to calculate what this density would be at 55°, that the quantities of acid and water may thereby be investigated.

To this end I took some dephlogisticated spirit of nitre, and examined its specific gravity at different degrees of heat, and found it as follows:

	Deg.		Sp. gravity.
at	30	-	1,4653
	46	-	1,4587
	86	-	1,4302
	120	-	1,4123

Therefore, the total expansion of this spirit of nitre from 30 to 120°, that is, by 90° of heat, was 0,0527; for 1,4650 − 1,4123 = ,0527, by which we see that the dilatations are nearly proportional to the degrees of heat: for beginning with the first dilatation from 30 to 46°, that is, by 16° of heat ∴ 90 . 0,0527 :: 16 . 0,0093; but in reality these 16° of heat afforded a dilatation equal only to 0,0063; for 1,4650 − 1,4587 = 0,0063; so that the difference betwixt the calculated and observed dilatations is only $\frac{30}{10000}$, a difference of no consequence in the present case, and that might arise from the immersion of the cold glass ball filled with mercury in the liquor, it being the solid I use to try the specific gravity of liquids. In the next case the difference is still less; for ∴ 90 . 0,0527 :: 56 . 0,0327; but 56° of heat produced in reality a dilatation of 0,0348 for 1,4650 − 1,4302 = 0,0348, so that the calculation is deficient only in $\frac{21}{10000}$.

I afterwards tried another, and somewhat stronger, spirit of nitre, whose specific gravity was

	Deg.	Sp. gravity.
at	34	1,4750
	49	1,4863
	150	1,5792

Here also the expansions are nearly proportional to the degrees of heat; for 116° of heat (the difference between 34 and 150) produce an expansion of 0,0958; and 15° of heat (the difference between 34 and 49) produce an expansion of 0,0097, and by calculation 0,0123, which last differs from the truth only by $\frac{26}{1000}$.

By this experiment we see, that the stronger the spirit of nitre is, the more it is expanded by the same degree of heat: for if the spirit of nitre of the last experiment were expanded in the same proportion as in the first, its dilatation by 116° of heat should be 0,0679, whereas it was found to be 0,0958.

As the dilatation of spirit of nitre is far greater than that of water by the same degree of heat, and as it consists only of acid and water, it clearly follows, that its superior dilatability must be owing to the acid part; and hence, the more acid is contained in a given quantity of spirit of nitre, the greater is its dilatability. We might therefore suppose, that the dilatation of spirit of nitre was intermediate betwixt that of the quantity of water it contains and that of its quantity of acid; but there exists another power also which prevents this simple result, namely, the mutual attraction of the acid and water to each other, which makes them occupy a less space than the sum of their joint volumes, which condensation I have therefore called their *accrued density*. Taking this into the account,

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we may consider *the dilatation of spirit of nitre as equal to those of the quantities of water and acid it contains minus the condensation they acquire from their mutual attraction, and this rule holds as to all other heterogeneous compounds.*

To find the quantities of acid and water in spirit of nitre, whose specific gravity was found in degrees of temperature different from those for which the table was constructed, *viz.* 54, 55, or 56° of FAHRENHEIT, the surest method is to find how much that spirit of nitre is expanded or condensed by a greater or lesser degree of heat, and then, by the rule of proportion, find what its density would be at 55°; but if this cannot be done, we shall approach pretty near the truth, if we allow $\frac{1}{1000}$ for every 15° of heat above or below 55° of FAHRENHEIT, when the specific gravity of spirit is between 1,400 and 1,500; and $\frac{1}{1000}$ when the specific gravity is between 1,400 and 1,300.

As to oil and spirit of vitriol I found the dilatations exceeding irregular, probably by reason of a white foreign matter, which is more or less suspended or dissolved in it, according to its greater or lesser dilution. This matter I would not separate, as I intended trying the density of this substance in the state in which it is commonly used. In general I found, that 15° of heat cause a difference of about $\frac{1}{1000}$ in its specific gravity when it exceeds 1,800; and of $\frac{1}{000}$ when its specific gravity is between 1, 400 and 1,300, its dilatation is greater than that of water, and so much greater as it is stronger.

The dilatations of spirit of salt are very nearly proportional to the degrees of heat, as appears by the following table.

	Deg.		Sp. gravity.
at	33	-	1,1916
	54	-	1,1860
	66	-	1,1820
	128	-	1,1631

Hence $\frac{5}{1000}$ ſhould be added or ſubtracted for every 21° above or below 55° in order to reduce it to 55°, the degree for which its proportion of acid and water was calculated. The dilatability of this acid is much greater than that of water, and even than that of the nitrous acid of the ſame denſity.

I now proceed to examine the quantity of pure acids taken up at the point of ſaturation by the various ſubſtances they unite with.

OF THE MINERAL ALKALI.

That which I made uſe of was procured from Mr. TURNER, who by a peculiar and ingenious proceſs extracts it in the greateſt purity form common ſalt.

Of this alkali I rendered a portion tolerably cauſtic in the uſual manner, and evaporating 1 oz. of the cauſtic ſolution to perfect dryneſs, I found it to contain 20,25 gr. of ſolid matter. I was aſſured, that the watery part alone exhaled during the evaporation, as the quantity of fixed air contained in it was very ſmall, and to diſſipate this a much greater heat would be requiſite than that which I uſed. This dry alkali I immediately diſſolved in twice its weight of water, and ſaturating it with dilute vitriolic acid, found it to contain 2,25 gr. of fixed air, that being the weight which the ſaturated ſo-

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lution wanted of being equal to the joint weights of the water, alkali, and ſpirit of vitriol employed.

The quantity of mere vitriolic acid neceſſary to ſaturate 100 gr. of pure mineral alkali I found to be 60 or 61 gr. the ſaturated ſolution, thus formed, being evaporated to perfect dryneſs weighed 36,5 gr.; but of this weight only 28,38 were alkali and acid, therefore the remainder, that is, 8,12 gr. were water. Hence, 100 gr. of GLAUBER's ſalt, perfectly dried, contain 29,12 of mere vitriolic acid, 48,6 of mere alkali, and 22,28 of water; but GLAUBER's ſalt cryſtallized contains a much larger proportion of water; for 100 gr. of theſe cryſtals being heated red-hot loſt 55 gr. of their weight. This loſs I ſuppoſe to ariſe merely from the evaporation of the watery part, and the remaining 45 contained alkali, water, and acid, in the ſame proportion as the 100 gr. of GLAUBER's ſalt, perfectly dried, abovementioned; then theſe 45 contained 13,19 gr. of vitriolic acid, 21,87 of fixed alkali, and 9,94 of water; conſequently 100 gr. of cryſtallized GLAUBER's ſalt contain 13,19 of vitriolic acid, 21,87 of alkali, and 64,94 of water.

I alſo ſaturated this alkali with the dephlogiſticated nitrous acid, and found that 100 gr. of the alkali took up 57 of the mere nitrous acid in the experiment I moſt depended on; but this quantity varied in ſome experiments a few grains, being ſometimes 60, and ſometimes 63 gr.; ſo that I conclude the proportion of this acid, taken up by the alkali, is nearly the ſame as that of the vitriolic acid. Suppoſing this quantity to be 57 gr. then 100 gr. Cubic nitre, perfectly dry, contain 30 of acid, 52,18 of alkali, and 17,82 of water; but Cubic nitre cryſtallized contains ſomething more water; for 100 gr. of theſe cryſtals loſe about 4 by gentle drying; therefore 100 gr. of

of the cryſtallized ſalt contain 28,8 of acid, 50,09 of alkali, and 21,11 of water.

Of mere marine acid, 100 gr. of this alkali required from 63 to 66 or 67 gr.; perhaps one reaſon of this variety is, that it is exceeding hard to hit the true point of ſaturation. Allowing it to be 66 gr. then 100 gr. of perfectly dry *common ſalt* contain nearly 35 of real acid, 53 of alkali, and 13 of water; but 100 gr. of the cryſtallized ſalt loſe 5 by evaporation; then 100 gr. of theſe cryſtals contain 33,3 of acid, 50 of alkali, and 16,7 of water.

The proportion of fixed air, alkali, and water, in cryſtallized mineral alkali, I inveſtigated thus: 200 gr. of theſe cryſtals were diſſolved in 240 of water; the ſolution was ſaturated by ſuch a quantity of ſpirit of nitre as contained 40 of mere nitrous acid; hence I inferred, that theſe 200 gr. of alkali contained 70 of real alkali. The ſaturate ſolution weighed 40 gr. leſs than the ſum of its original weight, and that of the ſpirit of nitre added to it; therefore it loſt 40 gr. of fixed air. The remainder, therefore, of the original weight of the cryſtals muſt have been water, that is, 90 gr.; conſequently 100 gr. of theſe cryſtals contained 35 of alkali, 20 of fixed air, and 45 of water.

This proportion is, particularly with regard to the alkali, very different from that found by Mr. BERGMAN and LAVOISIER, which I impute to their having uſed ſoda recently cryſtallized. Mine had been made ſome months, and probably loſt much water and fixed air by evaporation, which altered the proportion of the whole. According to the calculation of theſe philoſophers 100 gr. of this alkali takes up 80 of fixed air.

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The specific gravity of the crystallized mineral alkali weighed in æther I found to be 1,421.

OF THE VOLATILE ALKALI.

It is not possible by the old chymical methods to find the proportion of the ingredients in volatile alkalies, whether in a liquid or in a concrete state; seeing that, though it may be separated from fixed air, yet it cannot from water, on account of its extreme volatility. Then to find this proportion we must recur to the experiments of Dr. PRIESTLEY, who by his new analysis produced this alkali free from the aërial acid and water in the form of air: and in the third volume of his Observations, p. 294. informs us, that 1° measures of alkaline air take up, and are saturated by, 1 measure of fixed air. Let us suppose the measure to contain 100 cubic inches; then 185 cubic inches of alkaline air take up 100 of fixed air; but 185 cubic inches of alkaline air weigh, at a medium, 42,55 gr.; and 100 cubic inches of fixed air weigh 57 gr.; then 100 gr. of pure volatile alkali, free from water, take up 134 of fixed air.

On expelling its aërial acid from a parcel of this alkali in a concrete state, and formed by sublimation, I found 100 gr. of it to contain 53 of fixed air, and therefore, according to the preceding reasoning, 39,47 of real alkali and 7,53 of water *per cent.*

Saturating a solution of this alkali with the vitriolic, nitrous, and marine acids, I found, that 100 gr. of the mere alkali take up 106 of mere vitriolic acid, 115 of the nitrous, and 130 of the marine.

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The ſpecific gravity of the concrete volatile alkali weighed in æther was 1,4076.

The proportion of water in the different ammoniacal ſalts I have not been able to find, on account of their volatility; but believe it to be very ſmall, as volatile alkali and fixed air cryſtallize without the help of water, when both are in an aërial ſtate.

OF CALCAREOUS EARTH.

I firſt diſſolved this earth in the nitrous acid, and found that, after allowing for the loſs of fixed air and the quantity of water I formerly mentioned, 100 gr. of the pure earth take up 104 of mere nitrous acid. Inſtead of diſſolving this earth immediately in the vitriolic acid, I precipitated its ſolution in the nitrous by the gradual addition of the vitriolic, and found that to effect this 91 or 92 gr. only of mere vitriolic acid were required.

100 gr. of this pure earth demand for their ſolution 112 of mere marine acid. The ſolution, which is at firſt colourleſs, grows greeniſh on ſtanding. Natural Gypſum varies in its proportion of acid, earth, and water, 100 gr. of it containing from 32 to 34 of acid, and alſo of earth, and from 26 to 32 of water. The artificial contains 32 of earth, 29,44 of acid, and 38,56 of water; when well dried it loſes about 24 of water, and therefore contains 42 of earth, 39 of acid, and 19 of water *per cent.*

100 gr. *nitrous ſelenite*, carefully dried, contain 33,28 of acid, 32 of earth, and 34,72 of water.

100 gr. *marine ſelenite*, well dried, ſo as to loſe no part of the acid, contain 42,56 of acid, 38 of earth, and 19,44 of water.

OF

OF MAGNESIA OR MURIATIC EARTH.

This earth, perfectly dry and free from fixed air, could not be dissolved in any of the acids without heat. In the temperature of the atmosphere even the strongest nitrous acid did not act upon it in twenty-four hours; but in a heat of 180° these acids diluted with four or six times their quantity of water attacked it very sensibly; but as much of the acids is dissipated by heat, I could not judge of the exact quantity of acid requisite to dissolve a given quantity of it, any otherwise than by precipitating the solutions by another substance, whose capacity for taking up acids was known. The substance I used was a tolerably caustic vegetable alkali. By this method I found, that 100 gr. of pure magnesia take up 125 gr. of mere vitriolic acid, 132 of the nitrous, and 140 of the marine. None of these solutions reddened vegetable blues; all of them appeared to contain something gelatinous; that in the marine acid became greenish on standing for some time.

100 gr. of perfectly dry Epsom salt contain 45,67 of mere vitriolic acid, 36,54 of pure earth, and 17,83 of water; but 100 gr. of crystallized Epsom lose 48 by drying, and consequently contain 23,75 of acid, 19 of earth, and 57,25 of water. Common Epsom salt contains an excess of acid, for its solution reddens vegetable blues.

100 gr. of nitrous Epsom, well dried, contain 35,64 of acid, 27 of pure earth, and 37,36 of water.

The solution of marine Epsom cannot be tolerably dried without losing much of its acid, together with the water.

The specific gravity of pure muriatic earth is 2,3296.

OF EARTH OF ALLUM OR ARGILLACEOUS EARTH.

This earth I found to contain about 26 *per cent.* of fixed air, though I had previously kept it red-hot for half an hour: this surprised me much, as most writers say it contains scarce any. It dissolved in acids with a moderate effervescence until the heat was raised to 220°, after which I found the solution lighter than the quantities employed in the proportion I mentioned.

100 gr. of this earth (exclusive of the fixed air) require 133 of the *mere* vitriolic acid to dissolve them. This solution I made in a very dilute spirit of vitriol, whose specific gravity was 1,093, in which the proportion of acid to that of water was nearly as 1 to 14. This solution contained a slight excess of acid, turning vegetable blues into a brownish red; but it crystallized when cold, and the crystals were of the form of allum; so that I believe this to be nearly the proper proportion of its acid and earth; but there was not water enough to form large crystals. As this solution contained an excess of acid, I added more earth to it, but could not prevent its tinging blue paper red, until it formed an insoluble salt, that is, one that required an exceeding large quantity of water to dissolve it, and while part was thus become insoluble, yet another part would still retain an excess of acid; so that at the same time part would be supersaturated with earth, and another with acid, if tinging vegetable blues be a mark of an excess of acidity, which indeed in this case seems dubious.

100 gr. of Allum, perfectly dried, contain 42,74 of acid, 32,14 of earth, and 25,02 of water; but crystallized allum loses 44 *per cent* by deficcation; therefore 100 gr. of it contain 23,94 acid, 18 of earth, and 58,06 of water.

100 gr. of this pure earth take up as far as I can judge 153 of the mere nitrous acid. The solution still reddened vegetable blues; but after the addition of this quantity of pure earth, I think it was, that an insoluble salt came to be formed. The solution, when cold, grew turbid, and could not be wholly dissolved by 500 times its weight of water.

The same quantity of pure earth requires 173,45 of the mere marine acid for its solution; but the solution still reddens vegetable blues. After this an insoluble salt was formed; but the beginning of its formation is difficultly discovered both in this and in the former cases.

The specific gravity of argillaceous earth, containing 25 *per cent.* of fixed air, I found to be 1,9901.

OF PHLOGISTON.

Before I proceed to investigate its proportion in various compounds, and particularly in phlogisticated acids, it will be necessary to say something of its nature.

It is allowed on all hands, that fixed air, or the Aërial Acid, as it is more properly called, is capable of existing in two states; the one fixed, concrete, and unelastic, as when it is actually combined with calcareous earth, alkalies, or magnesia; the other, fluid, elastic, and aëriform, as when it is actually disengaged from all combination. In its concrete and unelastic state it can never be produced single and disengaged from other substances; for the moment it is separated from them, it assumes its aërial and elastic form. The same thing may be said of phlogiston: it can never be produced in a *concrete state*, single and uncombined with other substances; for the instant it

is disengaged from them, it appears in a fluid and elastic form, and is then commonly called *inflammable air*. These different states of the same substance arise, according to the immortal discoveries of Dr. BLACK, from the different portions of elementary fire contained in such substance, and absorbed by it, whilst its sensible heat remains the same, and hence called its *specific fire*. For want of attention to these different states, the very existence of phlogiston as a distinct principle has been frequently called in question, and chemists have been required to exhibit it separate in its fixed state, without recollecting, that neither can fixed air be shewn separate in a concrete state, nor that phlogiston may also be in the same predicament; while others have totally mistaken the nature of inflammable air, and imagined it to be a combination of acid and phlogiston. The reason why fixed air cannot be separated from any substance in a concrete state is, because when it is separated, for instance by means of an acid, there is always a double decomposition, the acid yielding its specific quantity of fire to the concrete fixed air, which then assumes an aerial form, while the fixed air yields the substance it was combined with to the acid. This is so true, that though a solution of lime in the nitrous acid yields a considerable quantity of heat, yet a solution of chalk in that acid scarcely yields any; for all the fire that is set loose, and rendered sensible in the first case, is absorbed by the fixed air in the second case, being precisely that which converts it into an aerial form. The separation of phlogiston from a metallic earth in the form of inflammable air arises from the same cause, the dissolving acid yielding its fire to the phlogiston, which then assumes an aerial form, while the phlogiston yields the metallic earth to the acid. It is true, that much sensible heat is produced on this occasion, for which

three

three ſubſtantial reaſons may be aſſigned; firſt, the proportion of fixed air in a given weight of crude calcareous earth, is much greater than that of phlogiſton in any metal, as will hereafter be ſhewn, it being in the former one-third of the whole, and that of phlogiſton in the latter for the moſt part not even one-ſixth. Secondly, much of the phlogiſton combines with the acid itſelf during the ſolution, and expels part of its ſpecific quantity of fire, as Dr. CRAWFORD has ſhewn, and as I have ſince experienced; and this fire muſt occaſion ſenſible heat. Thirdly, much of the phlogiſton, during ſolution, unites to the ſurrounding atmoſphere, expelling alſo part of its ſpecific fire, and this alſo muſt occaſion ſenſible heat; and hence it is, that metallic ſolutions *in vacuo* are generally attended with leſs heat; though with a more violent effervefcence than in open air. The ſolution of metallic calces is not attended with as much heat as that of their reſpective metals, not only becauſe neither the diſſolving acids nor the ſurrounding air is much phlogiſticated; but alſo becauſe they contain an elaſtic fluid in a concrete ſtate, which abſorbs much of the fire given out by the diſſolving acids, as it acquires an aërial ſtate.

The origin and formation of inflammable air being thus explained, I now proceed to ſhew its identity and homogeneity with phlogiſton. By phlogiſton is generally underſtood that principle in combuſtible bodies on which their inflammability principally depends; that principle to which metals owe their malleability and ſplendor; that which combined with vitriolic acid forms ſulphur; that which diminiſhes reſpirable air. Now inflammable air is that very principle which alone is truly inflammable, as Mr. VOLTA has elegantly ſhewn. In effect, combuſtible ſubſtances are either animal or vegetable,

as horns, hair, greaſe, wood, &c.; from all of which Dr. HALES has extracted inflammable air; or charcoal, from which Mr. FONTANA has extracted it, as did Dr. PRIESTLEY from reſins, ſpirit of wine, and æther, in all which it is the only principle that is inflammable, and they are inflammable only in proportion as they yield it; or phoſphorus, from whoſe acid Dr. PRIESTLEY has obtained this air by means of minium, for it was the acid, and not the minium, that contained it, as Dr. PRIESTLEY rightly conjectured, the acid obtained by deliqueſcence being never thoroughly dephlogiſticated until heated and vitrified, as Mr. MARGRAAF has ſhewn; or they are mineral ſubſtances, as ſulphur, from which inflammable air has been ſeparated by means of fixed alkalies, and, according to Dr. PRIESTLEY, alſo by means of marine air, or bitumens or bituminous ſubſtances, all of which may be made to yield it; or metallic ſubſtances, as zinc and regulus of arſenic, both of which are inflammable; but neither of them is ſo when deprived of its inflammable air: this is, therefore, the true and only principle of inflammability in any ſubſtance. I acknowledge that the inflammable air, proceeding from almoſt all theſe ſubſtances, is exceeding impure; that it contains from ſome a mixture of aërial acid or of oil, and from all ſome part of the ſubſtance which yields it or expels it, and hence its ſmell is different, according to the claſs of the ſubſtances from which it is extracted; but it is equally true, that none of theſe ſubſtances contribute to its inflammability; on the contrary, it is ſo much the leſs inflammable (that is, requires ſo much more air to be mixed with it before it flames) as it contains more of theſe heterogeneous ſubſtances. Hence inflammable air of the moraſſes is never totally conſumed*; and, on the contrary,

* 15 Roz. 146.

inflam-

inflammable air, from metals which is the purest of all, is also the most inflammable.

Secondly, Inflammable air is also the principle which reduces metallic earths to a metallic state, and gives them their metallic splendor. This has been proved analytically and synthetically, and therefore may be said to be as completely demonstrated as any thing in natural philosophy: thus Dr. PRIESTLEY has extracted inflammable air from iron and zinc by heat alone; and the iron, thus stripped of its phlogiston, lost its splendor, and was of a black colour, which is that which iron, slightly dephlogisticated, always assumes, as appears by *martial æthiops*: so also zinc and regulus of arsenic, when once inflamed, lose their metallic appearance: so also a mixture of lead and tin inflames in a moderate heat, and then both are converted into a calx destitute of splendor and malleability. On the other hand, if a current of inflammable air, in the act of combustion, be directed on the calces of iron, lead, or mercury, they are immediately revived and restored to their metallic form, as appears by the experiment of Mr. CHAUSSIER *. The following experiment is still more conclusive: if a polished plate of iron be put into a saturate and dilute solution of copper in the vitriolic or marine acids (I mention these because they are commonly used for the production of inflammable air, though the result is the same when other acids are used), no effervescence will arise, no inflammable air will be caught; but the iron will be dissolved, and the copper precipitated in its metallic form. Here inflammable air must be produced as usual, for the acid quits the copper and dissolves the iron; but this inflammable air instantly loses its aerial form, and unites to the copper, just as fixed air leaves alkalies to unite to lime

* 10 Roz. 313.

without

without any effervescence; and by this ſame inflammable air is the copper evidently reduced, acquiring ſplendor, malleability, and every other metallic property. But if the ſolution of copper be not ſaturated with copper, a ſmall quantity of inflammable air may be caught, as the exceſs of acid will diſengage more of it from the iron than the calx of copper can take up. Inflammable air is then the principle that metallizes metallic earth; and if metals contain only a ſpecific earth and phlogiſton, inflammable air certainly contains nothing elſe but phlogiſton. If iron and the arſenical acid be digeſted together, no inflammable air is produced; but the arſenical acid is, in great meaſure, converted into white arſenic, as Mr. BERGMAN has obſerved, and alſo Mr. SCHEELE*; what reaſon can be aſſigned why inflammable air is not produced by this as well as by all other acids; but that this metallic acid received it, and was by it reduced to a ſemi-metallic form, as by pure phlogiſton? Yet this acid produces inflammable air, from zinc becauſe zinc gives out more phlogiſton than the regulus of arſenic can take up; but it attracts and is metallized by a part of it, and it is only the exceſs that appears in the form of inflammable air, as Mr. SCHEELE has remarked. This inflammable air, indeed, is not pure, for it holds ſome of the regulus in ſolution; but this portion of regulus does not enter into its compoſition, as is very evident.

Thirdly, Inflammable air is the ſubſtance which, with vitriolic acid, forms ſulphur, for it is the very ſubſtance which the vitriolic acid ſeparates from metals; and this ſubſtance, ſo ſeparated, when in ſufficient quantity, and in proper circumſtances, unites to it in ſuch proportion as to form common ſulphur. Thus ſulphur is formed by diſtilling concentrated vitriolic acid

* 2 Nov. Act. Upſal. p. 210. Kon. Veten. Accad. Handlingar, vol. 36. p. 288.

with

with iron or bismuth, or by distilling tartar vitriolate with regulus of antimony. It is this also that diminishes respirable air, as Dr. PRIESTLEY has clearly shewn in the 5th vol. of his Observations, p. 84.; for though in its complete aërial state, after it has absorbed that large quantity of fire requisite to its aërial form, it difficultly and slowly unites to respirable air in the heat of the atmosphere, their points of contact through their difference of density being very small, and there being no substance at hand to receive the large portion of elementary fire they both contain, and of which they must lose a large proportion before they can combine together; yet while inflammable air is (as Dr. PRIESTLEY elegantly expresses it) in its *nascent* state, before it acquires its whole quantity of specific fire, respirable air easily unites to it, and is diminished in proportion to its purity; but if to a mixture of both, igneous particles of sufficient density to be visible be introduced, a degree of heat is excited, which, as it rarifies the dephlogisticated part of respirable air to a greater degree than it can inflammable air *, brings both into nearer contact, increases their attraction to each other, and both uniting give out their fire, or in other words *inflame*, when in proper proportion to each other, without any decomposition of either, unless the loss of a great part of their specific fire be called a decomposition, which loss is not usually called a decomposition; for water is never said to be decomposed when it becomes ice, nor metals when they become solid on cooling.

In answer to all this it will be said, that inflammable air undoubtedly contains phlogiston, which produces all the beforementioned effects; but that the phlogiston it contains is united to some other substance, which some will have to be an acid,

* 5 PRIEST. 359.

ſome an earth, and others reſpirable air. To theſe hypotheſes I ſhall oppoſe one general obſervation, which is, that ſince inflammable air, when pure, that is, when diſengaged from all heterogeneous ſubſtances which no way contribute to its inflammability, has always the ſame properties; it muſt, if it conſiſts of phlogiſton combined with any other ſubſtance, be always united to the ſame ſpecific ſubſtance; that is, if this be an acid, it muſt be always the ſame ſpecies of acid, or if an earth, it muſt be always the ſame ſpecies of earth; for we find, that ſubſtances, which are only *generically* the ſame, always produce, with any other given ſubſtance, compounds whoſe properties are very different from each other. Thus we ſee that the different ſpecies of alkalies, or earths, or metals, produce with one and the ſame ſpecies of acid compounds eſſentially different. This is a rule which, as far as I know, admits of no exception: and if we apply it to the abovementioned ſuppoſitions it will intirely deſtroy them; for it is impoſſible to think, that the phlogiſton can in every ſubſtance, that produces inflammable air, meet either the ſame acid, or earth, or any reſpirable air.

But to be more particular, the following reaſons demonſtrate that an acid of any ſort cannot be the baſis of inflammable air. 1ſt. Inflammable air has been, by Dr. PRIESTLEY, ſeparated from metals by mere heat. Now metals contain no acid, except perhaps their dephlogiſticated calx, which thoſe eminent chemiſts, BERGMAN and SCHEELE, ſuſpect to be of an acid nature; but theſe calces cannot enter into the compoſition of inflammable air, otherwiſe the inflammable air of each different metal would have different properties, as already ſhewn: nor indeed are theſe the acids that have been ſuppoſed to enter into the compoſition of inflammable air; but rather thoſe acids by

whoſe means it is extricated. But as this air is extricated from metals, not only by acids, but alſo by alkalies *, this ſuppoſition muſt vaniſh of courſe.

The ſame reaſons militate with equal ſtrength againſt the ſuppoſition that an earth of any kind enters into the compoſition of this air; nor is there an inſtance of any earth rendered permanently fluid by any means, except in ſparry air. Beſides, if it were a metallic earth, it muſt neceſſarily be ſuppoſed to be in a metallic ſtate; and how then could it eſcape the action of all kind of acids? for no acid is capable of decompounding inflammable air. Laſtly, reſpirable air cannot be ſaid to be the baſis of inflammable air, unleſs we ſuppoſe that reſpirable air enters into the compoſition of metals; for Dr. PRIESTLEY has, by ſolar heat, extracted inflammable air from them in a veſſel full of mercury, into which reſpirable air had no acceſs, and even *in vacuo*. Beſides, reſpirable air and phlogiſton form other compounds very different from inflammable air, *viz.* fixed and phlogiſticated airs as will preſently be ſeen.

It may alſo be fairly urged againſt all theſe ſuppoſitions, that they are not founded on any direct experiment, nor any known analogy, but merely gratuitous, or at leaſt deduced from experiments inadequate to their ſupport; whereas the opinion that inflammable air is nothing elſe than phlogiſton thrown into a fluid form by elementary fire, is directly founded on that experiment whereby inflammable air is ſeparated from metals by mere ſolar heat in the moſt perfect vacuum, juſt as fixed air united to marble and in a concrete ſtate (in which it is nearly of equal denſity with gold) is ſeparated from the marble, and thrown into a permanently fluid form by heat alone.

* Mem. Par. 1776, p. 687.

In favour of the exiſtence of an acid in inflammable air, it has been ſaid, that if this air be paſſed through water tinged blue by litmus, it reddens inſtantly. I have ſeen this frequently happen when inflammable air has been extracted from iron by ſpirit of vitriol; but if this air be waſhed, by paſſing it through lime-water, and then paſſed through, or agitated in, an infuſion of litmus, it will not diſcolour it in the leaſt: this I have ſeen done by Mr. FONTANA in June 1779. It has alſo been ſaid, that inflammable air and alkaline air, mixed together, form a cloud; but this has been fully diſproved by Dr. PRIESTLEY, in the fourth volume of his Obſervations.

That an earth of any kind is eſſentially requiſite to the conſtitution of inflammable air, ſeems to me utterly improbable; nor do I know of any experiment from whence it can be inferred. That metallic ſubſtances may be held in ſolution by inflammable air is certain; but it is equally ſo, that they no way contribute to its inflammability *, and are quite diſtinct from it.

But the opinion, that inflammable air conſiſts of reſpirable air ſuper-ſaturated with phlogiſton, is grounded on very ſpecious arguments drawn from experiments to be found in various parts of Dr. PRIESTLEY's works, which deſerve ſo much the more attention as the facts mentioned by that excellent philoſopher are not to be queſtioned. I ſhall endeavour to ſtate them with accuracy; but ſhall at the ſame time accompany them with ſuch remarks as ſeem to me to invalidate the concluſion that has been drawn from them.

In the firſt volume of Dr. PRIESTLEY's Obſervations it appears, that a quantity of ſtrong inflammable air, having been agitated in a glaſs jar immerſed in a trough of water,

* 2 PRIESTLEY, 268.

the ſurface of which was expoſed to the common atmoſphere, after the operation had continued ten minutes near one-fourth of the quantity had diſappeared; the remainder became fit for reſpiration, and yet was weakly inflammable. By further agitation it was diminiſhed half, and then admitted a candle to burn in it though feebly; but, on continuing the agitation a little longer, it came to extinguiſh a candle. Upon this I ſhall remark, firſt, that it clearly follows, from this experiment, that if the external reſpirable air had no acceſs to the inſide of the jar, half nearly of the inflammable air was converted into, or conſiſted of, reſpirable air, ſince ſuch quantity of air was found in it after the operation. Now it is abſolutely impoſſible that either could happen; for inflammable air could not be converted into half nor even one-third or one-fourth of its volume of reſpirable air, as even one-fourth of reſpirable air contains more matter than four times its bulk of inflammable air; it is then evident, that the external air muſt have had acceſs to it. Secondly, I agitated about half a pint of inflammable air, obtained from iron and previouſly paſſed through lime-water and kept over mercury, in about twelve times its bulk of water, out of which its air had been boiled in a glaſs bottle cloſed with a glaſs-ſtopper. The agitation continued at ſeveral times at leaſt two hours. A large quantity of the air was indeed abſorbed, as appeared by opening the bottle in water; but the remainder appeared, by the nitrous teſt, as noxious, and was alſo found to be as inflammable as at firſt. Even Dr. PRIESTLEY atteſts, that inflammable air, which had been united to water for one month, was afterwards as inflammable as ever. 3 PR. 267.

The true explanation of the firſt experiment appears, therefore, to be the following: Firſt, Water eaſily imbibes inflammable air, but does not combine with it; for after it has imbibed

bibed one-fourteenth of it, its taſte is no way altered, as Dr. PRIESTLEY has obſerved. 1 PR. 196. Water alſo eaſily imbibes common air: when, therefore, inflammable air is agitated in water having a communication with the atmoſphere, the inflammable air muſt neceſſarily be diminiſhed by reaſon of its abſorbtion, and the part ſo abſorbed immediately eſcapes out of the water into the atmoſphere, as is evident by the ſmell which is perceived when the quantity of inflammable air is conſiderable. This eſcape gives room for the further abſorbtion of the inflammable air which then eſcapes in the ſame manner. In the mean time the common air under the jar riſes into it, as appears by the direct experiments both of Dr. PRIESTLEY * and Mr. FONTANA; and hence the air in the jar muſt appear by the nitrous ſlightly phlogiſticated and reſpirable; but a further agitation will decompoſe the common air, as we ſhall ſoon ſee, and then a candle will be extinguiſhed. The ſame proceſs takes place when inflammable air ſtands long in water whoſe ſurface is expoſed to the atmoſphere.

Another experiment of the ſame tendency, but ſeemingly more deciſive, is to be found in the 4th vol. of Dr. PRIESTLEY's Obſervations, p. 368. There it is related, that a portion of inflammable air, incloſed in a glaſs tube, hermetically ſealed and heated until the glaſs was ſoftened, ſtained the glaſs black, and the tube being opened, the air was found reduced to one-third of its bulk; and this reſiduum was found to be mere *phlogiſticated air*, neither precipitating lime-water, nor being affected by nitrous air, or in the leaſt inflammable. Yet deciſive as this experiment appears, a little conſideration will ſhew the abſolute impoſſibility that inflammable air ſhould conſiſt of one-third phlogiſticated air and two-thirds phlogiſton;

* 1 PR. 96. 159. 3 PR. 156. Phil. Tranſ. 1776, p. 443.

for,

for, in the first place, one Cubic inch of phlogisticated air weighs 0,377 of a grain: now let us suppose, that to this phlogisticated air is added two-thirds of its bulk of phlogiston; and to make the supposition still stronger, let us also suppose, that phlogiston has no weight; then, by the supposition, this compound of phlogisticated air and phlogiston will constitute inflammable air, and amount to a bulk of three Cubic inches, and these three Cubic inches will weigh no more than 0,377 of a grain; but if three Cubic inches of inflammable air weigh 0,377 of a grain, one Cubic inch should weigh 0,105 of a grain, which cannot be; for then inflammable air would be little more than one-third lighter than common air, contrary to all the experiments that have been hitherto made, and particularly those of Mr. CAVENDISH, FONTANA, and Dr. PRIESTLEY himself, which shew it to be about eleven times lighter than common air. Secondly, It is said, that the matter which stained the glass black was the true phlogistic part of inflammable air, and was afterwards separated by means of minium. This then contained no phlogisticated air; but is it not certain, that if there had been enough of it, the minium would have been reduced and converted into lead? And might not inflammable air be again separated from that lead, though no phlogisticated or common air were at hand to supply its other supposed constituent part? Thirdly, In one of Dr. PRIESTLEY's experiments the inflammable air, contained in the glass tube which was most heated, was reduced to so small a bubble that no experiment could be made on it: therefore, in this, at least, the quantity of phlogisticated air did not amount to one-third, but was quite inconsiderable; the remainder then being taken up by the calx of lead in the glass, was pure mere phlogiston, so that this experiment is a strong proof of my opinion.

Fourthly,

Fourthly, If phlogiſton could be decompoſed by heat, and then leave a reſiduum of phlogiſticated air, amounting to one-third of its bulk, the diminution ariſing from its inflammation with common or dephlogiſticated air could never be ſo great as it is found to be by repeated experiments; for when inflammable and common air are fired in the proportion of eleven of the latter to four of the former, a bulk equal to the whole of the inflammable air, and to one-fifth of the common air, diſappears, according to Mr. VOLTA *, and the diminution is about two-fifths of the whole, or more exactly out of fifteen meaſures, only 8,8 remain; but if the inflammable air were decompoſed, and one-third of it, being phlogiſticated air, ſhould remain, then not quite one-fifth of the whole would vaniſh, and the reſiduum ſhould be 10,54 meaſures. This evidently proves, that pure inflammable air is never decompoſed (unleſs the loſs of its fire be called a decompoſition); but in the act of inflammation is totally transferred upon the pure part of reſpirable air to which it unites. Fifthly, To obtain ſtill a clearer inſight into this matter, I intreated Mr. CAVALLO, who is very expert in the management of the blow-pipe, as well as in pneumatic experiments, to repeat this experiment in my laboratory. We accordingly filled a tube 10,5 inches long, and one-fourth of an inch in diameter, with inflammable air from iron received over mercury, and having made the tube red-hot throughout and black, and ſoftened it ſo far as to endanger the eſcape of the air, we opened it on mercury. The air was diminiſhed only one-tenth, and inflamed with an exploſion as loud as an equal quantity of the ſame inflammable air that had not been heated.

* Roz. April 1779, p. 295.

The

The only queſtion that remains then is, whence the phlogiſticated air proceeded which Dr. PRIESTLEY mentions to have found? The circumſtance of his experiment would furniſh a plauſible anſwer; but the doctor has lately informed me, that he believes the air was really inflammable, but being a very ſmall quantity eſcaped before the flame could be applied.

It ſeems, therefore, ſufficiently proved, that inflammable air purified from the acids or other ſubſtances that expel it from its baſis, and alſo from all particles of the body to which it was originally united, ſuch as inflammable air from metals received on mercury, and well waſhed in lime-water, is one and the ſame ſubſtance with phlogiſton, differing only in quantity of fire, inflammable air containing nearly the ſame quantity of this element as the ſame bulk of atmoſpheric air, as Dr. CRAWFORD has found by ſome late experiments, an account of which will ſoon be laid before the public. This does not contradict that moſt important diſcovery of this ingenious philoſopher, that fire and phlogiſton repel each other: the meaning of this being only, that the addition of phlogiſton to any ſubſtance, as to reſpirable air, dephlogiſticated acids, metallic calces, expels part of the fire already contained in ſuch ſubſtance; and, on the contrary, by the removal of phlogiſton from any ſubſtance, the quantity of fire abſorbed by ſuch ſubſtance is increaſed.

It may appear extraordinary, ſuppoſing inflammable air and phlogiſton to be the ſame ſubſtance, that inflammable air ſhould mix ſo eaſily with water, whereas phlogiſton conſtantly repels and is repelled by it; but this intirely depends on the ſtate of this ſame ſubſtance, which, when fixed and concrete, is called *phlogiſton*, and, when rarified and aëriform, *inflammable air*. In this latter ſtate it mixes with water in proportion to its rare-

faction, as it even does in the leſs denſe forms of its concrete ſtate: thus æther is totally abſorbed by ten times its weight of water. The animal oil of Dippel mixes intirely with water; ſo does pure Petrol, and eſſential oils frequently diſtilled, and the ſpiritus rector of plants.

Much more remains to be ſaid of the different ſtates of phlogiſton from its moſt rarefied known ſtate, *viz.* that of inflammable air, to its moſt condenſed ſtate, that in which it is combined with metallic earths, &c. I have already diſtinguiſhed eight intermediate ſtates each differing from the other by the portion of elementary fire they contain, this quantity being, as far I can judge directly, as the rarefaction of the phlogiſton; but theſe reſearches are foreign to my preſent ſubject. I ſhall only remark, that phlogiſton, in a ſtate perhaps 100 times rarer than inflammable air, and conſequently containing much more fire, may poſſibly conſtitute the electric fluid.

P. S. Since I wrote the above, I have been honoured with a letter from Dr. PRIESTLEY, in which he informs me, that he has reduced the calces of iron, copper, lead, and tin, merely by melting them in inflammable air by means of a burning glaſs. A certain quantity of inflammable air was abſorbed by each during their reduction; but the unabſorbed part was equally inflammable, ſo that there was no decompoſition; but the remainder was of the ſame nature as the part abſorbed. He alſo, by the ſame means, converted nitrous vapour into nitrous air, and the phoſphoric acid into phoſphorus. And ſince the communication of the laſt mentioned experiments, which ſeem to him alſo a direct proof of the identity of inflammable air and phlogiſton, he has been ſo obliging as to inform me, that he has revived the calces of metals in *alkaline air* as well as

in *inflammable air*, and alſo formed a phoſphorus; and that he has little doubt but that he ſhall be able to produce any thing elſe in which phlogiſton is ſuppoſed to be concerned. This, he ſays, agrees with ſeveral of his former experiments, eſpecially that in which he produces inflammable air from alkaline air, by means of the electric ſpark and volatile alkali from iron, ſuperſaturated with phlogiſton by means of nitrous air, which he has repeatedly done ſince the publication of his laſt volume. This obſervation, he adds, may help to explain ſome things in the theory of chemiſtry, eſpecially the affinity which all acids have both with *phlogiſton* and with *alkalies*; but, he ſays, that alkaline air contains ſomething elſe beſides phlogiſton; becauſe when this air is uſed, there is always a reſiduum of ſomething that is *neither alkaline nor inflammable air*; but he wants more ſunſhine to complete and extend his experiments on this ſubject *.

OF THE QUANTITY OF PHLOGISTON IN NITROUS AIR.

100 gr. of filings of iron being diſſolved in a ſufficient quantity of very dilute vitriolic acid produced, with the aſſiſtance of heat gradually applied, 155 cubic inches of inflammable air, the barometer at 29,5, and the thermometer between 50 and 60°. Now inflammable air and phlogiſton being the ſame thing, this quantity of inflammable air amounts to 5,42 gr. of phlogiſton.

Again, 100 gr. of iron, diſſolved in dephlogiſticated nitrous acid, in a heat gradually applied and raiſed to the utmoſt, afford 83,87 cubic inches of nitrous air. And as this nitrous air con-

* Since this paper was committed to the preſs, I find that Mr. PELLETIER has reduced the arſenical acid to a regulus, by merely paſſing inflammable air through the ſolution of that acid in twice its weight of water. Ædr. Journ. February 1782.

tains nearly the whole quantity of phlogiſton which iron will part with (it being more completely dephlogiſticated by this acid than by any other means) it follows, that 83,87 cubic inches of nitrous air contain at leaſt 5,42 gr. of phlogiſton; but it may reaſonably be thought that the whole quantity of phlogiſton which iron will part with is not expelled by the vitriolic acid, and that nitrous acid may expel and take up more of it. To try whether this was really ſo, I calcined a certain quantity of green vitriol, until its ferruginous baſis was quite inſipid; I then extracted from 64 gr. of this ochre two cubic inches of nitrous air, conſequently 100 gr. of this ochre would give 3,12 cubic inches of nitrous air; and if 83,87 cubic inches of nitrous air contain 5,42 of phlogiſton, then 3,12 cubic inches of this air contain 0,2 of a grain of phlogiſton; conſequently, nitrous acid extracts from 100 gr. of iron two tenths of a grain more phlogiſton than the vitriolic acid does; therefore 83,87 cubic inches of nitrous air, containing nearly all the phlogiſton which iron gives out, contain 5,62 gr. of phlogiſton.

Then 100 *cubic inches of nitrous air contain* 6,7 *gr. of phlogiſton*, and ſince 100 cubic inches of nitrous air weigh 39,9 gr. they muſt alſo contain 33,2 *gr. of nitrous acid.*

Alſo, 100 *gr. of nitrous air contain* 16,792 *of phlogiſton, and* 83,208 *of acid.*

When firſt I made theſe experiments I imagined, that the nitrous air thus expelled contained all the phlogiſton of the metals diſſolved in the nitrous acid, as this acid is well known to dephlogiſticate metals as perfectly as poſſible; but I ſoon obſerved, as did Dr. PRIESTLEY and Mr. FONTANA, that the greater part of this is air reſorbed and detained in the ſolution, the acid and calx having, according to the beautiful remark of Mr. SCHEELE, a greater attraction to phlogiſton than either ſeparately; yet that the calculation

calculation is nearly juſt, will appear clearly in my next paper, by its coincidence with the quantity of phlogiſton diſcovered in lead by Dr. PRIESTLEY and that which is contained very evidently in regulus of arſenic, ſilver, and quickſilver.

OF THE QUANTITY OF PHLOGISTON IN FIXED AIR.

Before I attempt to determine this quantity, it will be neceſſary to prove that it contains any; and for this purpoſe minutely to examine its nature and origin.

Dr. PRIESTLEY firſt diſcovered that in all proceſſes, wherein phlogiſton is diſengaged from any ſubſtance, as in *combuſtion*, *reſpiration*, *calcination of metals*, *putrefaction*, *decompoſition of nitrous air by reſpirable air*, &c. fixed air is precipitated from the common or dephlogiſticated air in which theſe proceſſes are performed, and that theſe laſt airs are diminiſhed both in weight and bulk, and are afterwards leſs fit, or abſolutely unfit, for theſe proceſſes, according to the quantity of phlogiſton that was ſet looſe. Theſe facts are admitted by all, let their ſyſtems be what they may. However, Dr. PRIESTLEY thinks he has ſeen one exception to this general rule; for, he ſays, that in the combuſtion of inflammable and common air no fixed air is precipitated, 5 PR. 124. He alſo ſeems inclined to admit another exception in the caſe of the combuſtion of ſulphur.

The queſtions that here ariſe are, firſt, whether the fixed air that appears in theſe circumſtances proceeded from the reſpirable air or not? Secondly, If it proceeded from the reſpirable air, whether it pre-exiſted in that air; or whether it was generated

generated during the process that exhibits it? and if so, what are its constituent parts?

The first question is easily answered; for in such phlogistic processes as are attended with the destruction of the substances that are known to contain fixed air, as those of the animal and vegetable kingdom, the fixed air may be supposed to proceed in many cases, both from the decomposed substance and from the respirable air; and of this sort are the processes of combustion of most animal and vegetable substances, and fermentation; but the fixed air, that appears in such phlogistic processes as are performed on substances that contain no fixed air, must be deemed to proceed from the respirable air singly. And of this case we have four clear instances; the calcination of metals; the decomposition of nitrous air by respirable air; the diminution of common air by the electric spark; and, lastly, its diminution by amalgamation.

And first as to the calcination of metals, Dr. PRIESTLEY has observed, that by this operation respirable air (and only respirable air) is diminished between one-fourth and one-fifth, both in its weight and bulk; but Mr. LAVOISIER has demonstrated, that nothing is lost or escapes through the vessels (as Mr. SCHEELE would have it); for the weight and materials continue undiminished when the operation is performed in close vessels *. That part, therefore, which the air loses is taken up by the metallic calx, which accordingly is found to gain the very weight which the air loses. Now the air contained in the calx is fixed air; for Mr. LAVOISIER also observed, that by the calcination of lead, by solar heat, over lime-water, the water was rendered slightly turbid †. It is true,

* Mem. Par. 1774.

† 1 LAVOIS. 291.

that

that Dr. PRIESTLEY, in a similar experiment, did not observe this turbidity; but he accounts for this circumstance very justly, by supposing, that the calx of lead absorbed the fixed air preferably to the lime. And this supposition is not gratuitous; for metallic calces, and particularly those of lead, are known to attract fixed air as strongly as quick lime, or rather more strongly *: and, what sets this matter beyond all doubt, the calces of lead all yield fixed air by heat, and the grey calx of lead, in particular, which was that produced by Dr. PRIESTLEY, in the experiment to which I allude, affords by heat fixed air only. Other calces of lead after fixed air afford also dephlogisticated air; but this I shall shew also to have been originally fixed air. If filings of iron be mixed with water in close vessels, they will be converted into rust, and the incumbent air diminished one-fourth, as Mr. LAVOISIER attests †; but Dr. PRIESTLEY has shewn, that rust of iron yields scarce any other than fixed air, which may be expelled out of it by mere heat ‡. Nay, iron alone, exposed to common air over a vessel of water for three months, reduced this air one-fifth; and being exposed to dephlogisticated air, over a vessel of mercury, it reduced it one-tenth in nine months §. In all these cases the fixed air could surely come from nothing else but the incumbent respirable air and the phlogiston of the metal.

Secondly, It is well known, that if nitrous air be decomposed by respirable air over lime-water, the lime will be precipitated ||. In this case also, the fixed air must proceed from the

* VOGEL, § 599. 2 N. Act. Upsal. 240. IX Mem. Scav. Etrang. 544.

† 1 LAVOIS. 192.

‡ 2 PR. 112.

§ 2 PR. 182. 4 PR. 253.

|| 1 PR. 114. 3 PR. 30. 1 PR. 138.

respirable

reſpirable air and the phlogiſton of the nitrous air; for it cannot proceed from the nitrous acid, as this acid is not decompoſed, but is taken up by the water over which the mixture of both airs is made, as Mr. BEWLY has undeniably proved: and hence it is, that unleſs a large quantity of lime-water be uſed ſo as to contain enough for both the nitrous and aërial acids to act on, there will be no precipitation of lime, as Mr. FONTANA has obſerved; for the nitrous acid will ſeize on the lime preferably to the aërial. Dr. PRIESTLEY indeed obſerved, that if a bladder, filled with nitrous air, be dipped in lime-water, it occaſions a precipitation of lime on the ſurface of the water. 1 PR. 213. But he elſewhere acknowledges, that this proceeds from the inability of the bladder to confine nitrous air. 1 PR. 76. and 128, which Mr. BAUME alſo long ago obſerved, without knowing any thing more of this air. BAUME *ſur l'Æther*, 285. The phlogiſton paſſes through the bladder, and unites to the common air contiguous to it*. Beſides, nitrous air acts on the bladder itſelf, and extracts fixed air from it. 1 PR. 214. Hence alſo, if rain-water carefully boiled, and freed from its own air, be made to abſorb a quantity of nitrous air, it will again, on boiling, yield it back as pure as at firſt; but if common water be made to imbibe nitrous air in the ſame manner, it will, on boiling, yield alſo a portion of fixed air. 3 PR. 109. Does not this happen clearly becauſe common water contains atmoſpheric air, or air ſomewhat purer, which is converted into fixed air by mixture with the nitrous air? This experiment alſo ſhews, that water itſelf never unites to phlogiſton, ſince it does not take any from nitrous air, where the union of phlogiſton to the acid is of the laxeſt kind.

* 3 PR. 156.

Thirdly, If the electric ſpark be taken through common air, this air will be diminiſhed one-fourth, and a ſolution of lime, if contiguous, will be precipitated, and a ſolution of turnſole tinged red. 1 PR. 184. 186. Whence could the fixed air here produced proceed, but from the common air, and the phlogiſton of the metallic conductors? This excellent philoſopher has even ſhewn it could proceed from nothing elſe; for after that air had contributed all it could to that production, that is, was diminiſhed to the utmoſt, he changed the liquors, but could produce no change in their colour, nor the leaſt ſign of fixed air. This experiment has alſo been repeated in France, and the inſide of the glaſs tube, in which the common air was contained, was moiſtened with a ſolution of cauſtic fixed alkali, and the alkali, after the operation, was found cryſtallized; but when the tube was exhauſted of air, and the experiment repeated, no change whatſoever was found in the alkali. *Eſſai ſur l'Electricité, par Mr. Le Comte* DE LA CEPEDE, vol. I. p. 155.

Fourthly, If lead and mercury be agitated in a phial, partly filled with common air, this air will be diminiſhed one-fourth, and the reſiduum will be found completely phlogiſticated. The diminution will be ſtill greater if the phial contain dephlogiſticated air. 1 PR. 149. The lead is converted into a calx, calcination being the known effect of the amalgamation of the baſe metals; and this calx abſorbed the fixed air produced, for Dr. PRIESTLEY expelled this air from it. 1 PR. 144.; and hence an amalgama of lead and mercury decrepitates when heated*. Whence could this fixed air proceed, but from the reſpirable air? For ſurely neither lead nor mercury contain any.

* 1 MALOUIN. 105.

If the above experiments be attended to, the anſwer to the ſecond queſtion will be equally obvious. It is certain, that common air does not conſiſt of one-fourth of its bulk of fixed air; for if it did, the remaining three-fourths muſt be dephlogiſticated air: and if ſo, then the abſolute weight of a mixture of three-fourths dephlogiſticated air and one-fourth fixed air ſhould coincide at leaſt nearly with the abſolute weight of an equal bulk of common air; but in fact it is very far from it: for four cubic inches of common air weighed 1,54 gr.; but a mixture of three cubic inches of dephlogiſticated air and one of fixed air weighs 1,83 gr.; neither indeed has ſo large a portion of fixed air been ever ſuppoſed to exiſt in common air. Beſides, if fixed air pre-exiſted in common air, it might be ſeparated from it by lime-water, at leaſt in ſome degree. I have mixed one part of fixed air with twenty of dephlogiſticated air, and alſo with twenty of phlogiſticated air in cloſe veſſels, and theſe mixtures did not fail to render lime-water turbid. But let common air be agitated in lime-water ever ſo long in cloſe veſſels, not the leaſt cloudineſs will appear; nor does quick-lime, in theſe circumſtances, in the leaſt affect common air, as Dr. PRIESTLEY has obſerved. 2 PR. 184. The ſpontaneous precipitation of lime-water ariſes therefore from an accidental diffuſion of fixed air through common air, and the ſlowneſs of this precipitation ſhews its quantity to be very ſmall. The inference from the above experiments will be much ſtronger againſt the pre-exiſtence of fixed air in reſpirable air, if, inſtead of common air, dephlogiſticated air be uſed; for there the diminution is ſo great, and the quantity of fixed air produced ſo conſiderable, that it can by no means be ſuppoſed to have pre-exiſted, its properties being ſo very oppoſite to thoſe of dephlogiſticated air.

To

To this it has been anſwered, firſt, that fixed air in common air is united to ſome unknown baſis, which attracts it more ſtrongly than quick-lime does; but that it is precipitated from that baſis by the phlogiſton ſet looſe in phlogiſtic proceſſes, which is ſtill more ſtrongly attracted by that baſis; and, ſecondly, that the diminution both of the weight and bulk of reſpirable air in phlogiſtic proceſſes does not ariſe intirely from the ſeparation of fixed air, but from ſome other cauſe.

But neither of theſe anſwers is ſatisfactory: for the ſuppoſition of ſuch a baſis is evidently gratuitous, being ſupported by not one experiment. It is alſo contrary to analogy, there being no inſtance of the ſeparation of fixed air, nor of any other acid, from any ſubſtance merely by the greater affinity of phlogiſton to ſuch ſubſtance. It is alſo inſufficient for the purpoſe for which it was framed; for of dephlogiſticated air 97 parts in 100 are reducible to fixed air by phlogiſtic proceſſes; and can it be imagined, that 97 parts in 100 of it were mere fixed air united to leſs than three parts of an unknown baſis? I ſay, leſs than three parts; for, according to the preſent ſuppoſition, this unknown baſis took up the phlogiſton of the ſubſtance that ſeparated the fixed air from it, and yet it, and the whole quantity of phlogiſton it took up, amounted but to three parts of an hundred; can it be ſuppoſed, that this vaſt proportion of fixed air would not in the leaſt affect lime-water, as pure dephlogiſticated air is known not to do? Can it be ſuppoſed, that ſuch an immenſe quantity of fixed air, combined with any baſis, would be ſo ſuperlatively fitted for all phlogiſtic proceſſes, while fixed air, in its diſengaged ſtate, is totally unfit for them? Beſides, this unknown baſis, after all, is nothing but phlogiſticated air, with which fixed air is incapable of contracting any union; and if its phlogiſton be waſhed away, it is not

found different from common air ſlightly injured. Accordingly, we find that this conjecture, firſt advanced by Dr. PRIESTLEY in the infancy of his reſearches, is now abandoned by him. Vol. V. p. 31. And he now juſtly thinks, that common air does not contain above $\frac{1}{12}$ of its bulk of fixed air.

As to the diminution of bulk, it is certain, that the whole of it does not proceed from the ſeparation of fixed air; for though no part of the fixed air ſhould be abſorbed, yet ſince part of the common air is converted into fixed air, there muſt be a diminution of bulk, ſince fixed air is ſpecifically heavier than common air, and the bulks are inverſely as the ſpecific gravities; but the diminution of *maſs* muſt wholly, and that of bulk muſt alſo for the greater part ariſe from the abſorption of fixed air by water, or the ſubſtance from which the phlogiſton proceeds. I have ſucceſſively added ſix meaſures of nitrous air to two of dephlogiſticated air from precipitate *per ſe*, and after each addition transferred the mixture into freſh lime-water, and after each I found the lime precipitated until the whole was reduced to one-tenth nearly, ſo that nine-tenths of this dephlogiſticated air was evidently converted into fixed air; and ſince fixed air did not pre-exiſt in the dephlogiſticated air, it was evidently produced by the union of the phlogiſton of the nitrous air with the truly dephlogiſticated part of the dephlogiſticated air.

Here we ſee how fixed air is generated in moſt other phlogiſtic proceſſes, performed in common air. The phlogiſton is attracted by the dephlogiſticated part of common air, unites to it, expels part of its fire, and ſo forms fixed air; yet a part of this pure air generally eſcapes the action of phlogiſton, being protected from it by the quantity of phlogiſticated air which is always found in common air, and which forms about two-thirds of it, in the ſame manner as gold is protected by

ſilver,

ſilver, and ſilver by gold, from the action of their reſpective menſtruums; and this is the reaſon why, in ſome phlogiſtic proceſſes, the diminution is greater than in others; and why the diminution continues to increaſe ſlowly for a long time.

Nor is the ſuppoſition, that common air conſiſts of two fluids, one phlogiſticated and the other dephlogiſticated, gratuitous; it is pointed out by ſeveral experiments. If a mixture be made of three parts phlogiſticated air and one of dephlogiſticated air, it will exactly perform the functions of common air; a candle will burn in it, an animal will live in it, juſt as in common air *. Beſides, common air may in ſome meaſure be ſeparated into theſe conſtituent parts by lying over pure water; for dephlogiſticated air is much more miſcible with water than common air, as Mr. FONTANA remarked, Phil Tranſ. 1779, p. 443. and 444 †, and SCHEELE on Fire, § 94. Hence, if common air be ſuffered to ſtand ſome time over pure water, it will be diminiſhed, the purer part being in great meaſure abſorbed by the water, and the remainder will be found to conſiſt of ſo large a proportion of phlogiſticated air that a candle will not burn in it. 1 PR. 158. 4 PR. 353. Mr. SCHEELE again expelled that part which the water had abſorbed, and found it dephlogiſticated. He alſo found, that phlogiſticated air is not at all abſorbed by water. ibid.

Hence we ſee, why the whole of any quantity of common air can never be converted into fixed air; for no part of it will unite with phlogiſton, but the dephlogiſticated part (which never exceeds one-third of the whole). This Mr. SCHEELE has deciſively proved by expoſing liver of ſulphur to a mixture

* Mem. Par. 1777, p. 191.

† He informed me, that water takes up one-fourteenth of its bulk of dephlogiſticated air, and only one-twenty-eighth of common air.

of

of phlogiſticated and dephlogiſticated air; the mixture was diminiſhed in the ſame proportion as it contained dephlogiſticated air, and no more. SCHEELE, § 43.

Phlogiſticated air, therefore, is not the uſual product of common phlogiſtic proceſſes; but the phlogiſticated reſiduum that is found after ſuch proceſſes muſt have pre-exiſted, as that evidently does which is found after the mixture of nitrous and very pure dephlogiſticated air, for almoſt the whole of this laſt is turned into air which is abſorbed by water, and precipitates lime, as we have already ſeen, ſo that no part of it is converted into phlogiſticated air, this being immiſcible with water. Now common air is affected by nitrous air juſt in the ſame manner, and differs only in degree; therefore the phlogiſticated air, which is found after its phlogiſtication in the uſual proceſſes, was not produced by thoſe operations, but pre-exiſted.

Phlogiſticated air conſiſts of fixed air ſuper-ſaturated with phlogiſton, as ſulphur does of volatile vitriolic acid ſuper-ſaturated with phlogiſton; and as ſulphur is not generally formed when the vitriolic acid unites to phlogiſton, but only volatile vitriolic acid, ſo neither is phlogiſticated air each time that pure air unites to phlogiſton, but rather fixed air, I ſay *ſuper-ſaturated*, becauſe it contains ſuch a quantity of phlogiſton as to be inſoluble in water. Many experiments of Dr. PRIESTLEY clearly point out this compoſition. Thus that celebrated philoſopher has found, that if phlogiſticated air be agitated in water, out of which its air had been boiled, and whoſe ſurface is expoſed to the atmoſphere, it will be in great meaſure purified (juſt as ſulphur is decompoſed by trituration in water), and if then it be paſſed through lime-water two or three times, it renders it turbid. 2 PR. 218. Here then the exceſs of phlogiſton, by reaſon of its repulſion from water, is

eaſily

eaſily attracted by the dephlogiſticated part of the common atmoſphere, which is immediately imbibed by the water out of which its air had been boiled; the phlogiſticated air is thereby decompoſed and partly turned into fixed air, which makes the lime-water turbid. Part alſo of the fixed air is decompoſed as will preſently be ſeen, and hence the degree of purity which it acquires. Further, if the electric ſpark be taken in fixed air, three-fourths of it will be rendered inſoluble in water, and the whole will probably become ſo if the operation be long enough continued. 1 PR. 248. This inſoluble reſiduum Mr. FONTANA found to be phlogiſticated air; and that, if this phlogiſticated air be agitated in water (whoſe ſurface is expoſed to the atmoſphere) it will become again common air. *Recherches Phyſiques*, 77. That is, it will acquire a degree of purity nearly the ſame as that of common air. This fully confirms all that has been hitherto ſaid concerning theſe airs; ſo alſo, if a mixture of filings of iron and ſulphur, made into a paſte, be expoſed to fixed air, and made to ferment, part of the fixed air will be turned into phlogiſticated air, 3 PR. 257. Juſt as in another equally curious experiment he found, that vitriolic air was converted into ſulphur by the gradual exhalation of phlogiſton from a ſolution of that air in water, and as it daily happens in the hot baths at Aix la Chapelle. And hence we ſee, that fixed air, even in its elaſtic ſtate, is capable of taking an exceſs of phlogiſton, when this laſt is inſenſibly ſeparated from any ſubſtance, and then becomes phlogiſticated air. Phlogiſticated air may alſo be formed by a rapid and copious affluence of phlogiſton, in certain circumſtances, as we ſhall ſoon ſee. I ſhould not omit, that phlogiſticated air, after it has been purified from phlogiſton by agitation in water, is again diminiſhable by phlogiſtic proceſſes, and that fixed air is preci-

pitated from it as uſual. 2 PR. 219. A circumſtance which at that time was thought inexplicable, and which indeed is ſo, on any other principles but thoſe here laid down, of which it is an immediate conſequence.

Having thus far ſynthetically proved the conſtituent parts of fixed air to be pure elementary air and phlogiſton, I ſhall now endeavour to do the ſame by its analyſis: and, in the firſt place, that it contains phlogiſton, and even in ſuch quantity as to deſerve to be claſſed among the phlogiſticated acids, appears by its action on *black manganeſe*. This ſemi-metallic calx, as has been proved by that admirable chemiſt Mr. SCHEELE, is completely ſoluble only in phlogiſticated acids, and is precipitable from them by fixed alkalies in the form of a white calx. He alſo found, that this manganeſe is alſo ſoluble in water ſtrongly impregnated with fixed air, and is alſo precipitable from it in the form of a *white calx*. 35 Mem. Stock. p. 96.

If fixed air be repeatedly diſſolved in, and expelled from water, it leaves each time a reſiduum which is inſoluble in water, diminiſhable by nitrous air, and capable of ſupporting animal life. Hence it is evidently decompoſed, the phlogiſton ſeparating from it, and gradually uniting to the common atmoſphere by reaſon of the repulſive power betwixt it and water. Dr. PRIESTLEY indeed found, that a candle would not burn in it; but this ariſes only from a mixture of a ſmall quantity of fixed air not yet decompoſed, of which, according to the experiments of Mr. CAVENDISH, one-ninth is ſufficient to extinguiſh a candle *.

Again, Mr. ACHARD has converted fixed air into air of nearly the ſame purity as common air by paſſing it five or ſix times through melted nitre. Mem. Berlin. 1778. Mr. CAVALLO

* 1 PR. 34. 40. 2 PR. 219, 220.

passed it but once through melted nitre, and yet found it considerably meliorated, for it was diminished by nitrous air. In this case the nitrous acid attracted the phlogiston; for it is known to become phlogisticated by the fusion of nitre, so as to be expellable even by the vegetable acids. 2 N. Act. Upſ. 171. And aqua regia may be made by mixing nitre with marine acid.

I shall now briefly consider what may be said in opposition to this doctrine.

In the first place it may be difficult to conceive, that the addition of phlogiston should render any substance more soluble in water, as it is known to render most acids less soluble in that liquid; but a little attention will shew, that phlogiston does not always render substances less soluble in water than they were before; for the acid of sugar is less soluble in water than sugar itself, though sugar consists of that acid united to phlogiston. The dephlogisticated marine acid unites more difficultly with water than the same acid does when phlogisticated, as the illustrious BERGMAN has observed*. Caustic volatile alkali has been decomposed by Mr. SCHEELE, and found to consist of an air insoluble in water, and phlogiston; so that it is rendered soluble in water only by union with phlogiston. It would be foreign to the subject to enter into the reason of these exceptions, but the facts are certain.

Another objection may be drawn from a remarkable experiment to be found in the fifth volume of Dr. PRIESTLEY's observations, where it is said, that inflammable air and common air being fired by an electric spark over lime-water, the diminution took place all once, and the lime was not precipitated; but as it is equally true, that fixed air is precipitated by other phlogistic processes, this experiment proves only, that in these

* Anleitung, § 333.

particular circumſtances, where a large quantity of phlogiſton is ſuddenly heated and transferred all at once upon the dephlogiſticated part of common air, phlogiſticated air may be formed as ſulphur and volatile acid is formed, when a large quantity of hot phlogiſton is united all at once to the vitriolic acid.

Analogous to this is another experiment of Mr. CAVALLO's, where he found, that, by the exploſion of gunpowder, a large quantity of phlogiſticated air is produced *; and alſo another of Dr. PRIESTLEY's, wherein he found, that by firing a mixture of equal parts, ſulphur and nitre, only one-twelfth of the air produced was fixed air, the remainder being phlogiſticated air. But I own the circumſtances of the former experiment are not as yet well known to me, not having been able to repeat it in ſuch a manner as to remove all doubt either of the eſcape of the air through the cement which fixes the wire that conducts the electric fire, during combuſtion; or that the ſmall quantity of inflammable air uſed prevents the fixed from being ſenſible. It may alſo happen, that to the production of fixed air it is neceſſary that the phlogiſton be condenſed to a certain degree, as it is in common caſes; and perhaps, when exceedingly rarified, as it is in inflammable air from metals, it forms ſome other, as yet unknown, compound. Thus much is certain, that all other inflammable air, fired with the electric ſpark, produces fixed air; and all other inflammable air is ſpecifically heavier than the metallic, and before inflammation evidently contains no fixed air. Mr. WARLTIRE, after burning metallic inflammable air, found a *white powdery ſubſtance* (probably a calx) which may have abſorbed the fixed air.

However, in the common proceſs of combuſtion of animal and vegetable ſubſtances it is certain, that fixed air is ſeparated

* CAVALLO on Air, p. 512.

from

from common air, and that the whole diminution is owing to its production and abſorbtion. Mr. LAVOISIER has ſet theſe points in the cleareſt light. He introduced a lighted candle into a receiver ſtanding on mercury: the air at firſt expanded by reaſon of the heat, and the candle was ſhortly after extinguiſhed; but when all was cold, there was ſcarcely any diminution. He then introduced, under the receiver, a cauſtic fixed alkaline liquor: the air was immediately diminiſhed, and the diminution reached nearly one-ninth of the whole. He then introduced a ſmall quantity of vitriolic acid; the alkali immediately effervesced, gave out its fixed air, the mercury redeſcended, and the air in the receiver occupied the ſame ſpace as at firſt; ſo that this experiment is perfectly concluſive. He alſo lighted a candle in dephlogiſticated air, and when it was extinguiſhed, introduced a cauſtic fixed alkaline liquor, and then, and only then, this air was diminiſhed two-thirds, by which it is evident, that two-thirds of it were converted into fixed air; but the remaining third was ſo far from being phlogiſticated air, that a candle burned in it as well as ever, and after it went out half of this air was abſorbed by a cauſtic fixed alkali, and the remainder was ſtill little worſe than common air. Mem. Par. 1777, p. 195, &c.

Yet Mr. LAVOISIER thinks, that by the calcination of metals fixed air is not produced; but that the metals abſorb the dephlogiſticated part of common air, and are thereby converted into a calx. And on this is founded his extraordinary opinion of the non-exiſtence of phlogiſton; whereas it is evident, that even mercury affords inflammable air, and conſequently contains phlogiſton, and that it loſes part of this during calcination, and conſequently fixed air muſt be produced, as he himſelf acknowledges it to be during combuſtion, by the union of inflam-

mable air and the dephlogiſticated part of common air, which after this union is abſorbed by the calx. It is true, that the mercurial calx, and alſo the calces of lead, and many others, yield dephlogiſticated air; but then the mercury is always revived, ſo that it is evident, it retakes the phlogiſton from the fixed air, of which nothing then remains but the dephlogiſticated part, which accordingly appears in the form of dephlogiſticated air. Dr. PRIESTLEY never found the whole of the mercury revived, and accordingly he recovers a little fixed air from the mercurial calx. 2 PR. 217. But Mr. LAVOISIER finds the whole of the mercury revived, and for that reaſon finds no fixed but all dephlogiſticated air; thus their different reſults are clearly explained, and probably proceed from the different degrees of heat they employed, and the different phlogiſtication of their acids. The dephlogiſticated air that is extracted from *minium* proceeds alſo from a partial revivification of the lead, which always takes place*: nor is it wonderful, that this calx ſhould dephlogiſticate fixed air, ſince it dephlogiſticates the marine acid alſo, as Mr. SCHEELE has obſerved †.

To this it will probably be objected, that dephlogiſticated air muſt pre-exiſt in the *minium*, ſince it is expelled by the marine acid; but this does not follow; for if manganeſe be diſſolved in the common marine acid which is phlogiſticated, and afterwards expelled from it by the vitriolic, it will alſo be found dephlogiſticated.

I ſhall now proceed to inveſtigate the proportion of phlogiſton and elementary or reſpirable air in fixed air.

Dr. PRIESTLEY, in the fourth volume of his Obſervations, p. 380. has ſatisfactorily proved, that nitrous air *parts* with as much

* BEAUME, 7. 1 Pott. Lithog. 29. 3 Dict. Chy. 205.

† Kon. Vet. Acad. Handling. vol. XXXV. p. 193.

much phlogiſton to common air as an equal bulk of inflammable air does when fired in the ſame proportion of common air. Now, when inflammable air unites with common air, its whole weight unites to it, as it contains nothing elſe but pure phlogiſton; ſince, therefore, nitrous air phlogiſticates common air to the ſame degree that inflammable air does, it parts with a quantity of phlogiſton equal to the weight of a volume of inflammable air ſimilar to that of nitrous air. Now 100 cubic inches of inflammable air weigh 3,5 gr.; therefore, 100 cubic inches of nitrous air part with 3,5 gr. of phlogiſton when they communicate their phlogiſton to as much common air as will take it up. I ſay, that nitrous air *parts* with *as much* phlogiſton, becauſe it is certain, that it does not part with the *whole* of its phlogiſton to common or dephlogiſticated air, for it contains much more, as already ſhewn, and, as appears by the red colour, it conſtantly aſſumes when mixed with common or dephlogiſticated air, which colour belongs to the nitrous acid combined with its remaining phlogiſton, and not to the fixed air then produced, nor to the phlogiſticated air remaining, as is very evident. Hence the acid, thus formed, is volatile. 4 PR. 267.

One meaſure of the pureſt dephlogiſticated air and two of nitrous air occupy but $\frac{1}{100}$th parts of one meaſure, as Dr. PRIESTLEY has obſerved, vol. IV. p. 245. Suppoſe one meaſure to contain 100 cubic inches, then the whole very nearly of the nitrous air will diſappear, its acid uniting to the water over which the experiment is made, and 97 cubic inches of the dephlogiſticated air, which is converted into fixed air by its union with the phlogiſton of the nitrous air; therefore 97 cubic inches of dephlogiſticated air take up all the phlogiſton which 200 cubic inches of nitrous air will part with; and this we

have

have found to be ſeven grains; therefore, a weight of fixed air equal to that of 97 cubic inches of dephlogiſticated air and 7 of phlogiſton, will contain 7 gr. of phlogiſton. Now, 97 cubic inches of dephlogiſticated air weigh 40,74 gr.; to which, adding 7 gr. we have the whole weight of the fixed air equal 47,74 gr. = 83,755 cubic inches; and conſequently 100 *cubic inches of fixed air contain* 8,357 *gr. of phlogiſton, and the remainder elementary air.*

100 *gr. of fixed air contain* 14,661 *of phlogiſton and* 85,339 *of elementary air*; which, when ſtripped of phlogiſton, and impregnated with its proper proportion of elementary fire, becomes again dephlogiſticated air. Hence alſo, 100 cubic inches of dephlogiſticated air are converted into fixed air by 7,2165 gr. of phlogiſton, and will be then reduced to the bulk of 86,34 cubic inches.

And reciprocally, 100 cubic inches of fixed air, being decompoſed, will afford 115,821 cubic inches of dephlogiſticated air, and part with 7,2165 gr. of phlogiſton, ſuppoſing the decompoſition to be complete; that is, the dephlogiſticated air abſoutely pure.

Having read the foregoing account of the nature of fixed air to Dr. PRIESTLEY, I had the ſatisfaction to find it met with his entire approbation, which he authorized me to mention, notwithſtanding what he had advanced to the contrary in his laſt publication.

OF

OF THE QUANTITY OF PHLOGISTON IN VITRIOLIC AIR.

The method I pursued was this:

1st, I found the quantity of nitrous air a given weight of copper afforded when dissolved in the dephlogisticated nitrous acid, and by that means how much phlogiston it parts with.

2dly, I found the quantity of copper which a given quantity of the dephlogisticated vitriolic acid could dissolve; and observed, that it could not dissolve the greatest quantity of copper without dephlogisticating a further quantity which it does not dissolve.

3dly, I found how much it dephlogisticates what it thoroughly dissolves, and how much it dephlogisticates what it barely calcines.

4thly, How much inflammable air a given quantity of copper affords when dissolved in the vitriolic acid to the greatest advantage.

5thly, I deduct from the whole quantity of phlogiston expelled by the vitriolic acid the quantity of it contained in the inflammable air; the remainder shews the quantity of it contained in the vitriolic air.

The particulars were as follows:

1st, 100 gr. of copper dissolved in the dephlogisticated nitrous acid afforded me 67,5 cubic inches of nitrous air, which, according to the before mentioned calculation, contain 4,52 gr. of phlogiston.

2dly, 100 gr. of real vitriolic acid take up or dissolve 54,73 of copper, and 100 gr. of copper require about 182,714 gr. of

of real vitriolic acid to diſſolve them. Again, 100 gr. of copper, when diſſolved in the vitriolic acid, retain only as much phlogiſton as is contained in three cubic inches of nitrous air, that is, 0,2 of a grain; therefore, ſince 100 gr. of copper give out 4,52 of phlogiſton, the vitriolic acid ſtrips it of 4,52 − 0,2, that is, 4,32 gr. of phlogiſton.

3dly, To diſſolve 70 gr. of copper in the vitriolic acid, to the greateſt advantage, 20 more muſt be ſlightly dephlogiſticated; therefore, to diſſolve 100 gr. of copper in this acid, 28,6 more muſt be ſlightly dephlogiſticated. 8 grs. of this ſlightly dephlogiſticated calx afforded 4 cubic inches of nitrous air; therefore, 28,6 would afford 14,3, which contain 0,958 gr. of phlogiſton; but 28,6 gr. of copper, before any dephlogiſtication, contain 1,292 gr. of phlogiſton; therefore, they loſe by this ſlight dephlogiſtication 0,344 of a grain of phlogiſton. Hence, when 100 gr. of copper are diſſolved in the vitriolic acid, the quantity of phlogiſton expelled is 4,32 + 0,34 = 4,66 gr.

4thly, The quantity of inflammable air afforded by the moſt advantageous ſolution of 100 gr. of copper in the vitriolic acid is 11 cubic inches, which amount to 0,385 of a grain of phlogiſton

5thly, The ſolution of 100 gr. of copper in the vitriolic acid afforded over mercury 75,71 cubic inches of air; but of this only 11 cubic inches were inflammable air, the remainder therefore was vitriolic acid air, amounting to 64,71 cubic inches.

6thly, Then the whole quantity of phlogiſton expelled during the ſolution of 100 gr. of copper in the vitriolic acid is 4,66 gr.; of this inflammable air contains but 0,385 of a grain: the remainder therefore, which conſiſts of 4,275 gr. muſt be con-

tained in the 64,71 cubic inches of vitriolic air: therefore, 100 *cubic inches of vitriolic air contain* 6,6 *gr. of phlogiſton, and* 71,2 *gr. of acid*, and 100 cubic inches of this air weighing 77,8 gr. 100 *gr. of this air contain* 8,48 *gr. of phlogiſton and* 91,52 *of acid.*

OF THE QUANTITY OF PHLOGISTON IN SULPHUR.

This I endeavoured to find by eſtimating the quantity of fixed air produced during its combuſtion.

To the top of a glaſs bell, which was open, I firmly tied and cemented a large bladder, deſtined to receive the air expanded by combuſtion, a quantity of which generally eſcapes when this precaution is not uſed. Under this bell, which contained about 3000 cubic inches of air, I placed a candle of ſulphur, weighing 347 gr.; its wick (which was not conſumed) weighed half a grain: it was ſupported by a very thin concave plate of tin, to prevent the ſulphur from flowing over during the combuſtion, and both were ſupported by an iron wire, fixed on a ſhelf in a tub of water. As ſoon as the ſulphur was fired with a very feeble flame, it was covered with the bell, the air being ſqueezed out of the bladder. The inſide of the bell was ſoon filled with white fumes, ſo that the flame could not be ſeen. In an hour after, the fumes thoroughly ſubſided, and all was cold. The water roſe within the bell to a height equal to 87,2 cubic inches; whence I deduce that 87,2 cubic inches of fixed air were produced, which contain 7,287 gr. of phlogiſton, which ſeparated from the vitriolic acid, and united to the dephlogiſticated part of the common air under the bell.

The candle of ſulphur being weighed, was found to have loſt 20,75 gr.; therefore, 20,75 gr. of ſulphur contain 7,287 gr. of phlogiſton, beſides the quantity of phlogiſton which remained in the vitriolic air. This air muſt have amounted to $20,75 - 7,287 = 13,463$ gr. which contain 1,141 gr. of phlogiſton; therefore, the whole quantity of phlogiſton in 20,75 gr. of ſulphur is 8,428 gr.; therefore, 100 *gr. of ſulphur contain* 40,61 *gr. of phlogiſton and* 59,39 *of vitriolic acid.*

Several attempts have hitherto been made to determine the proportion of the conſtituent parts of ſulphur; but all were evidently defective. The firſt was that of STAHL, who calculated the quantity of phlogiſton from that of the acid remaining after ſlow combuſtion; but as much, both of acid and phlogiſton, was diſſipated, and as the remaining acid was alſo phlogiſticated, and attracted much of the moiſture of the air, no concluſion whatever could be drawn from this experiment. The ſecond method was, to form a liver of ſulphur, and convert this by a gentle long continued heat into a tartar vitriolate, and then calculate the weight a given quantity of alkali would gain by this operation. This was alſo deviſed by STAHL, and followed by BRANDT and NEWMAN, and by it they determined the proportion of phlogiſton to that of acid to be nearly as 1 to 16. But during the formation of the liver of ſulphur, whether in the moiſt or dry way, much of the phlogiſton and acid is diſſipated, as is evident by the vapour and ſmell that proceed from it, their alkali alſo contained fixed air, which it loſt during the operation, and of which they kept no account, as they were ignorant of its exiſtence; and the tartar vitriol formed by them or ſal polycreſte retained much undecompoſed ſulphur, as always happens when it is not ſtrongly heated; ſo that this method alſo was very imperfect, however ſome ſub-

ſequent chemiſts who made the experiment with more care concluded from it, that ſulphur contained one-ſeventh of phlogiſton. EXLEBEN, § 760.

By weighing flowers of ſulphur in a perforated braſs box in water, I found its ſpecific gravity to be 1,924. It remained in the water a quarter of an hour before any air iſſued from it, and then ſome bubbles aroſe; but when I opened the box, I found the middle part of the flowers quite dry, ſo that I make no doubt but ſome air ſtill remained, and that its ſpecific gravity is ſtill greater. Mr. PETIT weighed it in oil, and found its ſpecific gravity 2,344, which I believe to be nearly the truth.

OF THE QUANTITY OF PHLOGISTON IN MARINE ACID AIR.

8 gr. of copper diſſolved in colourleſs ſpirit of ſalt afforded but 4,9 cubic inches of air, when the air was received over water, and this air was inflammable.

8,5 gr. of copper being diſſolved in the ſame quantity of the ſame ſpirit of ſalt, and the air received over mercury, afforded 91,28 cubic inches of air; but of theſe only 4,9 cubic inches were inflammable air; the remainder, therefore, *viz.* 86,38 were marine air, which weigh 56,49 gr.

Now, as ſpirit of ſalt certainly does not dephlogiſticate copper more than the vitriolic acid does, it follows, that theſe 4,9 cubic inches of inflammable air, and 86,38 cubic inches of marine air, do not contain more phlogiſton than would be ſeparated from the ſame quantity of copper by the vitriolic acid: and ſince 100 grains of copper would yield to the vitriolic

acid 4,32 gr. of phlogiſton, 8,5 gr. of copper would yield 0,367 of a grain of phlogiſton; this then is the whole quantity extracted by the marine acid, and contained in 91,28 cubic inches of air, and deducting from this the quantity of phlogiſton contained in 4,9 cubic inches of inflammable air (=0,171 of a grain), the remainder, *viz.* 0,367 − 0,171 = 0,196 is all the phlogiſton that can be found in 86,38 cubic inches of marine air. Then 100 *cubic inches of marine air can contain but* 0,227 *nearly of a grain of phlogiſton* 65,173 *of acid.*

Hence we ſee why it acts ſo feebly on oils, ſpirit of wine, &c. having a very ſmall affinity to phlogiſton; and why it is not diſlodged from any baſis by uniting with phlogiſton, as the vitriolic and nitrous acids are, its affinity to it being inconſiderable.

XVI. *Del modo di render ſenſibiliſſima la più debole Elettricità ſia Naturale, ſia Artificiale. By Mr.* Alexander Volta, *Profeſſor of Experimental Philoſophy in* Como, *&c. &c.; communicated by the Right Hon.* George *Earl* Cowper, *F. R. S.*

Read March 14, 1782.

1. UN apparecchio, che portando a uno ſtraordinario ingrandimento i ſegni elettrici fa ſi, che oſſervabile divenga e coſpicua quella virtù, che altrimenti per l'eſtrema ſua debolezza sfuggirebbe i noſtri ſenſi, ognun comprende di quale e quanto vantaggio ſia per riuſcire nelle ricerche ſull' elettricita, e maſſime intorno alla naturale atmosferica, la quale, come ſappiamo, non in ogni tempo, anzi aſſai di rado, allora ſolamente cioè che il cielo è ingombro di nuvoloni ſcuri e tempeſtoſi, avvienne che ci renda ſenſibile ne' conduttori ordinari non molto elevati, e appena è che in altri tempi ne moſtri qualche indizio in quelli elevatiſſimi, o ne' *cervi volanti* portati all' altezza di piu' centinaja di braccia. Or un tale apparecchio, mercè di cui un conduttore atmosferico, anche di non grande elevazione, vi dia ſegni ad ogn' ora e in ogni coſtituzione di tempo, molto chiari e diſtinti di quel qualſiſia picciolo elettrizzamento che in lui induce l'atmosfera, ecco io ve lo preſento nel mio elettroforo: in quella ſemplice macchina, che è ormai nelle mani di tutti, e che ſe altro pregio pur non aveſſe verrebbe abbaſtanza raccomandata agl' Elettriciſti per queſto che lor offre facile mezzo di ſpiare la più languida e impercettibile elettricità ſi naturale che artificiale, con tirarla ſopra di sè, ed accumularla

accumularla al punto di promoverne e invigorirne per ſingolar maniera i ſegni.

2. In vero ogni volta che queſti mancano nell' ordinario modo di ſperimentare, e ne ſcintilla ſcorgeſi nè cenno benché minimo di attraimento, il dire che pur vi ſia elettricità, fora un' aſſerzione gratuita, anzi un giudicare contro ogni apparenza. Malgrado queſto non poſſiamo neppur dire accertatamenta che punto non ve n' abbia: e il concluderlo da ciò ſolo che niun ſegno per anco ci ſi moſtra, è un precipitare il giudizio; imperocchè chi ci aſſicura che qualche elettricità ivi non ſi truovi realmente, ma coſi debole da non poter attrarre tampoco un legger filo? Or queſto è che c'importa in molti caſi di ſapere, ſpecialmente quando ſi tratta di elettricità naturale. Un conduttore atmoſferico poco elevato non dà ordinariamente ſegni, come già ſi è detto, che quando gli ſovraſta oſcuro nembo: a cielo coperto d'alte nubi ſparſe o diſteſe equabilmente, quando l'aria è ingombrata da nebbie, in tempo di pioggia placida ed anche dirotta, tranne qualche roveſcio improvviſo, raro è che ſcorger vi ſi poſſa alcun indizio di elettricità, o nulla mai a ciel ſereno, ſia placido, ſia ventoſo. Stando pertanto alle apparenze, e al giudizio di un elettroſcopio comune, anche dè più ſenſibili, direbbeſi che il conduttore non è elettrizzato punto, e che per conſeguenza non domina elettricità di ſorta ne' campi dell' aria poco alti ove quel conduttore porta la teſta. Eppure non è così: un altro elettroſcopio di gran lunga migliore qual veramente puo dirſi il noſtro apparechio, giacchè ne adempie con tanto vantaggio le funzioni, ci fa vedere che da qualche elettricità è pur ſempre inveſtito quel conduttore, avvegnache ne ſi moſtri di per ſe affatto inerte: ci fa, dico, vedere e toccar con mano ch' egli non ne è mai privo affatto; onde convien giudicare in egual modo che non ne è mai priva l'aria che lo circonda. Ed ecco come

reſtiamo

restiamo convinti che anche alla più bassa regione dell' atmosfera, e fino a pochi piedi da terra s'estende l'azion costante e perenne dell' elettricità naturale. Cotal elettricità sebbene insensibile rimanga finche da quel tratto d'atmosfera si comunica soltanto al detto conduttore; ove poi per mezzo di lui si comunichi insiememente all' elettroforo nostro, si raccorà entro a questo più facilmente, e in maggiore copia*; si è per tal modo, che sorger quindi potranno i noti segni di attrazzione e di ripulsione sensibili abbastanza per dinotarci senza equivoco non che l'esistenza, la specie ancora dell' elettricità, cioè se *positiva* o *negativa*. Che più? non mancherà talora di comparire per fino qualche scintilluzza. Ogniqualvolta poi il conduttore desse già di per se qualche segno, movendo alcun poco un legger filo, aspettatevi pure col' soccorso del' nostro apparecchio, scintille pungenti, e ogn' altro segno vigorosissimo.

3. Mà veniamo senza più al modo di far servire all' intento cotal apparecchio, a cui in questo caso meglio che il nome che altronde porta di elettroforo, l'altro già indicato di *elettroscopio*, anzi pure quello di micro-elettroscopio potrebbe convenire. Ma io amo meglio di chiamarlo *condensatore* dell' elettricità, per usare un termine semplice e piano, e che esprime a un tempo la ragione e il modo de fenomeni di cui si tratta, come vedrassi nella 2ª parte del presente scritto. Tutto dunque si riduce a queste poche operazioni.

(A) Convien prendere un piatto d'elettroforo, che abbia l'incrostatura di resina assai sottile, e a cui o non sia stata dianzi impressa alcuna elettricità, o se mai vi è stata, vi si sia spenta affatto.

(B) A questa faccia resinosa immune da ogni elettricità si sopraponga convenientemente il suo scudo (così io chiamo la

* Come ciò segua si spiegherà nella 2ª parte di questa memoria.

lamina

lamina ſuperiore dell' elettroforo): voglio dire le ſi applichi a combaciamento, e ſi collochi nel bel mezzo in modo, che non tocchi in alcun punto l'orlo metallico del piatto, ma rimanga iſolato.

(C) Coſi congiunti eſſendo ſi adattino ſotto al filo conduttore dell' elettricità atmosferica in guiſa, che lo ſcudo venga toccato dove che ſia dal detto filo, egli ſolo lo ſcudo, e in niun modo il piatto.

(D) In queſta ſituazione ſi laſcino le coſe per un certo tempo, finche lo ſcudo poſſa aver raccolta competente doſe di quell' elettricità, che dal filo conduttore gli s'iſtilla lentiſſimamente.

(E) Da ultimo ſottraggaſi al contatto e influſſo del filo conduttore lo ſcudo tuttavia unito al ſuo piatto; indi ſi diſgiunga anche da queſto, levandolo in alto al conſueto modo per il ſuo manico iſolante: e allora ſarà che ſe ne otterranno gl' aſpettati ſegni coſpicui di attrazione, di ripulſione, e di qualche ſcintilla eziandio, di pennoncelli &c. nel tempo che il conduttore di per ſe non giugna a moſtrar nulla, o appena un' ombra di elettricità.

4. Ho detto (prec. D) che il filo conduttore debbe toccare lo ſcudo per un certo tempo. Quanto però non è facile il determinarlo, dipendendo dalle circoſtanze. Talora vi abbiſogneranno 8. 10. e più minuti; quando cioè il conduttore da par ſe ſolo non fà vedere il minimo ſegno di elettricità: altre volte più poco. Che ſe un debole indizio pur vi compariſſe, tantoche un legger filo faceſſe cenno d'eſſerner attratto, baſteria in tal caſo laſciar in contatto di eſſo conduttore il noſtro ſcudo ſol pochi ſecondi, per abilitar queſto a dar ſegni molto vivaci.

5. Una coſa ſi vuol oſſervare riſpetto al filo conduttore medeſimo, ed è ch' egli ſia ben continuo, o ſe è poſſibile d'un pezzo ſolo dall' alto fino al baſſo, dove viene a comunicare collo ſcudo: cioè ſi dee evitare aſſolutamente ogni interruzione, e il più

più che si puo ancora la semplici giunture ad anello od uncino, per la ragione che ciascuna di tali giunture portando un qualche impedimento al passàgio dell' elettricità, avvenir puo che quella, che contrae il conduttore in alto, s'arresti, nè giunga al luogo desiderato, cioè fino allo scudo. Così succederà diffatti ogni qualvolta l'elettricità è deboliſſima, se in luogo d'un filo metallico continuo, una catena di più annelli da quello pendente venga a toccare cotesto scudo. Non si creda per questo che una sola giuntura o due possano egualmente impedire la riuscita; ma ne verra sempre del pregiudizio: e qualora l'elettricità fosse estremamente debole, potrebbe sì per l'indicato difetto mancare del tutto l'esperimento.

6. Riguardo all' elettroforo da adoperarsi altre osservazioni rimangono, di cui ora mi convien parlare. E la prima accennata sopra al § 3. (let. A) si è che lo strato resinoso importa molto che sia sottile, avendo io sempre provato che quanto più lo è tanto maggior dose di elettricità permette, anzi fa che si raccolga entro allo scudo cui porta indosso, di quell' elettricità, dico che gli s'infonde o dall' atmosfera per mezzo del filo conduttore, o da qualsivoglia altra potenza elettrica. Se fosse pertanto stesa la resina alla spessezza d'un quarto di linea, o non maggiore di una mano di varnice, riuscirebber le prove ottimamente; siccome all' incontro essendo grossa un pollice o più, andrebber le cose maliſſimo.

7. In secondo luogo la superficie di essa resina debb' essere quanto si puo piana e liscia, e piana e liscia similmente l'inferior faccia dello scudo, sicchè vengano a combaciarsi bene (ivi let. B). E noto quanto un miglior combaciamento favorisca gli effetti dell' elettroforo; ond' ebbi ben ragione di raccommandar questa come una delle principali condizioni nella descrizione che pub-

blicai a ſuo tempo di queſta machina *. Ma è ancor più grande l'influenza, che l'ampio e perfetto contatto ha ſopra l'iſteſſo apparecchio, allorchè il medeſimo agiſce in qualità di condenſatore.

8. Da ultimo merita particolar attenzione quanto alla già citata let. A ſi è preſcritto, che alla faccia reſinoſa, cui ſi applica lo ſcudo, non dee eſſere impreſſa alcuna elettricità. La ragione per cui vuolſi che ne ſia affatto priva ella è, che altrimenti i ſegni dello ſcudo, allorchè s'alza, diverrebbero equivoci, non eſſendo più la ſola elettricità trasfuſa in eſſo ſcudo dal conduttore atmoſferico quella che giuoca, ma inſieme anche l'altra occaſionata dall' elettricità impreſſa ed inerente alla faccia reſinoſa: quando a noi importa di eſplorare la ſola ſopraveniente la detto ſcudo.

Se dunque la faccia reſinoſa del piatto, di cui volete ſervirvi, è rimaſta ſempre intatta, va bene. Ma ſe è ſtata già eccitata, e vi ſi mantiene tuttavia parte dell' impreſſa elettricità, egli convien fare di tutto per iſpegnarla; cio che non è ſi agevol coſa. Il paſſarvi ſopra un panno alquanto umido, applicandolo ben bene a tutta la ſuperficie, è un de mezzi più efficaci ch' io mai abbia trovato †; pur non toglie talvolta che dopo qualche tempo lo ſcudo poſatovi ſopra, e previo il ſolito toccamento, rialzato, non attragga ſenſibilmente un filo. Lo ſteſſo ſuccede non di raro anche dopo aver tuffato tutto il piatto nell' acqua, laſciatovelo un pezzo, e quindi fattolo raſciugare all' aria. Lo ſquagliare la ſuperficie della reſina al fuoco o al ſole, è forſe il più ſicuro ſpediente per farne ſvanire tutta quanta l'elettricità,

* Si truova queſta deſcrizione in un colle principali eſperienze, e un piccol ſaggio di ſpiegazione, in due memorie indirizzate in forma di lettera al Dr. PRIESTLEY, e pubblicate in un' opera periodica di Milano intitolata *Scelta d'Opuſcoli intereſſanti*, per l'anno 1775.

† Vegg. l'accennata deſcrizione dell' elettroforo.

ſicchè

sicchè non ne rimanga pur ombra o vestigio nella stessa resina, rassodata che sia *. Una maniera più spedita è di far passare sopra tutta la faccia della resina la fiamma di una candela, o d'un foglio di carta acceso. A qualunque però di tai mezzi uno si appigli, per accertarsi che l'elettricità sia spenta a segno che più non possa aver parte alcuna l'azione propria dell' elettroforo agli effetti che risultar debbono unicamente dall' elettricità infusa allo scudo dal conduttore atmosferico, converrà far prima la prova se posato esso scudo sulla faccia resinosa, toccato col dito, e rialzato al consueto modo, non mova neppure un sottilissimo pelo: allora non producendo alcun effetto in qualità d'elettroforo, servirà ottimamente all' altr' uso, cui vien destinato, di condensatore dell' elettricità.

9. Se mi si dimandasse ora a qual grado giunga nel descritto apparecchio cotal condensazione dell' elettricità, cioè a quanto maggior forza sforger possano i segni elettrici nello scudo quando s' alza, risponderei che non è facile il determinarlo, dipendendo da molte circostanze. Che però, le altre cose pari, l'aumento è maggiore in ragione che il corpo che fornisce l'elettricità allo scudo si truova avere maggiore capacità; ed è più grande in proporzione che la forza elettrica im-

* E stato creduto per molto tempo che il calore, e molto più la liquefazione del solfo e delle resine, bastasse senz' altro ad eccitarvi l'elettricità. Ma tranne la tormalina, ed alcune altre pietre, che sí veramente concepiscono l'elettricità pel solo calore, le resine e il solfo non è mai che lo facciano, se loro non sopravvenga qualche stropicciamento, o tocco almeno d'altro corpo. L'errore è nato, come ha avvertito il Prof. BECCARIA con altri, da che ogni legger tocco della mano, o di checchè altro può bastare in tali circostanze favorevoli. Senza questo la materia fusa abbandonata a se stessa nel rapprendersi e dopo, tanto è lungi che contragga alcuna elettricità, che anzi perde quella qualunque che per sorte aver potesse prima della fusione, come le nostre sperienze ci assicurano. Nè fia meraviglia: giacchè tutti i corpi coibenti per un forte grado di calore divengono conduttori; e i corpi resinosi in ispecie lo sono già quando si trovan molto rammolliti, e più allorchè cominciano ad entrare in fusione.

piegata è più debole. Così vedemmo giù che ſe il conduttore atmoſferico non ha la forza di alzare d'un grado il pendolino dall' elettrometro, movendo appena un ſottil pelo, ed anche meno di queſto, potrà tuttavia abilitare lo ſcudo non che a vibrar l'elettrometro a più gradi alto, ma a ſcagliare pur anche vivace ſcintilla (§ 2. e ſeq.). Ma ſe l'elettricità nel conduttore atmoſferico ſarà più forte a ſegno di dare qualche ſcintilletta, di mandare l'elettrometro a 5.0,6 gradi, lo ſcudo che riceverà queſta elettricità, darà gli è vero una ſcintilla aſſai più forte, e l' elettrometro vibreraſſi al più alto punto eſ. gr. a 100 . 120 gradi. Ad ogni modo è viſibile che la condenſazione dell' elettricità ne in queſto é minore che nel p°. caſo, in cui venne aumentata ſi, ma non di 60 volte, la ragione è che al di la del maſſimo non ſi puo andare, cioè di quel grado a cui giunta l'elettricità ſi diſſipa da ſe ſteſſa aprendoſi il paſſagio per tutto. Dunque a miſura che la potenza elettrica, la quale ſi applica allo ſcudo poſato, è più vicina a tal ſommo grado, minor accreſcimento puo ricevere dall' apparecchio condenſatore. Ma che biſogno abbiamo noi allora di lui, e tutte le volte che l'elettricità è già ſenſibile e forte abbaſtanza ?

L'uſo a cui vien deſtinato è di ſottrarre, e raccolta ſopra di ſe ſufficiente doſe render ſenſibile quella, che è languida affatto e impercettibile, finchè rimane nel gran conduttore in pace (1.).

10. Quando dunque il conduttore vi dà già da ſe ſolo ſegni abbaſtanza diſtinti di elettricità, non accade ricorrere all' altro apparecchio. Dirò dippiù che il farlo puo produrre un grande inconveniente, ed è, che per poco che l'elettricità del conduttore ſia vigoroſa, a ſegno di dare qualche ſcintilla, avviene allora che facendogli toccare lo ſcudo, l'elettricità non ſi arreſti in lui ſolo, ma che paſſi in parte ad imprimerſi alla faccia reſinoſa cui copre ; onde in ſeguito l'apparecchio prenda a fare le

funzioni di vero elettroforo: ciò che per le ragioni già dette (8.) si dee con ogni studio evitare.

11. Per prevenire un tal inconveniente ho pensato di surrogare al piatto incrostato di resina, un piano che non fosse vero e perfetto isolante, assolutamente impermeabile al fluido elettrico; ma tale solamente che opponesse una discreta resistenza al suo passaggio, come una lastra di marmo asciutta e politissima, un piattello di legno similmente asciutto ed arido, oppure incrostato di gesso, o meglio ancora inverniciato, una tela incerata secca e monda, od altro simile. Alla superficie di tali corpi non avverrà d'ordinario che s'affigga l'elettricità potendo appiccata che sia scorrere e trapassare per entro ad essi; o se pur talvolta ve ne rimanesse un pocolino, quasi stagnante, sia questa passaggera, in brevi momenti svanita. Quindi è che un tal apparecchio inatto alle funzioni d'elettroforo non ce ne darà i fenomeni; ma per questo appunto meglio servirà all' altr' uso di condensatore.

12. Sostituendo così allo strato resinoso o a qualsivoglia altro coibente perfetto un piano o strato che sia mezzo tra coibente e deferente, cioè un corpo isolante molto imperfetto e insieme imperfettissimo conduttore, quali sono nelle divisate circostanze gl' indicati corpi (prec.) non solamente si toglie o si fa minore il pericolo di qualche elettricità che possa imprimersi e restar aderente alla superficie del piatto, la quale renderebbe equivoche le sperienze delicate; ma innoltre un notabile vantaggio da noi si ottiene, ed è che lo scudo posato su tai piani non affatto isolanti cava del conduttore, e si tira addosso maggior dose di elettricità, che se posato fosse sopra uno strato resinoso, od altro perfetto coibente. E come detto già abbiamo (6.) che uno strato resinoso quanto è men grosso, tanto più abilita la lamina che gli è sovrapposta ad arricchirsi di elettricità; così tale strato

ridotto

ridotto ad una ſemplice vernice, o intonaco di cera, l'una e l'altra già men coibente della reſina, e infine ridotto a niente, ſoſtituendovi ſoltanto una ſuperficie poco deferente, come quella del marmo o del legno arido, ſomminiſtrà alla lamina metallica la più favorevole politura che mai aver poſſa per raccogliere nel ſuo ſeno abondante elettricità.

13. Guardiamoci però nel voler iſchivare il troppo di coibenza di dare nel poco, accoſtandoci ai deferenti perfetti, o quaſi perfetti. Non biſogna perder di viſta, che la ſuperficie del piatto dee opporre una diſcreta reſiſtenza al trapaſſo del fluido elettrico, per rattenere una competente doſe di elettricità nello ſcudo addoſſatole (11.). Mà baſta che ciò faccia per un qualche picciolisſimo tempo; d'uopo eſſendo non rare volte di tenervi confinata l'elettricità otto, dieci, e più minuti, quanti cioè ne impiega il conduttore atmoſferico a raccoglier dall' aria, ed infondere in eſſo ſcudo tal copia di elettricità, che poſſa renderſi ſenſibile e coſpicua (3. D. 4.).

Dal che facilmente s'intende quanta attenzione pur convenga e nella ſcelta del corpo da ſurrogarſi allo ſtrato di reſina, e nella convenevole preparazione del medeſimo: la quale preparazione conſiſte generalmente in certo grado di eſſiccamento, che lo riduca allo ſtato di ſemicoibente nè più, nè meno. Ad ogni modo fia meglio peccare per eccesſo di coibenza, che per difetto; meglio prendere un piatto qualſivoglia incroſtato di reſina, che un deſco di legno nudo non aridiſſimo, una laſtra d'oſſo, od una di marmo comune non previamente riſcaldato al ſole o al fuoco: giacchè niun oſſo, e pochiſſimi tra i marmi ho trovato che valgano a tener confinata l'elettricità nello ſcudo che combaciano oltre ad un minuto o due al più, ſe abilitati non vengano da un convenevole riſcaldamento. Diſpoſti però che ſiano in tal modo, e ove ſingolarmente incontrata ſi fia ottima qualità

nel

nel marmo, rieſcono a meraviglia, e ſorpaſſano ogni aſpettazione; onde ſoſterrò ſempre con ragione, che ſi fatti piani di legno, d'oſſo, di pietra, nudi come ſono, e ancora notabilmente deferenti, meritano tuttavia d'eſſere preferiti a un ordinario piatto d'elettroforo fornito del ſuo ſtrato reſinoſo.

14. Venendo ora più davvicino alla maniera, onde praticamente ſi puo ridurre il noſtro apparecchio alla maggior perfezione, per ritrarne il più gran vantaggio, dopo aver ricordato come conviene ſoprattutto che lo ſcudo s'adatti bene a combaciamento col piano ſottopoſto (3 lat. B. e 7.), ſoggiugnerò che per ottener ció nel miglior modo é bene d'appigliarſi ad una laſtra di marmo, e queſta inſieme alla lamina o ſcudo metallico ſpianare ben bene, lavorandole una ſopra l'altra, finchè ſian ridotte a tale perfetto combaciamento, che ne naſca ſenſibile coeſione tre loro.

Il marmo poi coſi lavorato ſi eſponga per molti giorni al calore d'una ſtufa, con che eſpellendoſi l'umido di cui anche tali pietre ſono ſpeſſo inbevute, verrà il marmo condotto a queſto ſtato d'imperfettiſſimo conduttore, che è l'ottimo per le ſperienze di queſto genere (12. 13.); e ſi manterrà tale per un pezzo, ſol che non reſti lungamente eſpoſto al grand' umido: poichè per quell' umidore che puo contrarre accidentalmente, e in poco tempo, non eſſendo che ſuperficiale, non verrà eſſo marmo a deteriorarſi notabilmente; e baſterà prima di ſperimentare eſporlo per alcuni minuti al ſole, o pur anche aſciugarlo con un pannolino caldo.

15. E quì giova avvertir di nuovo, che non tutti i marmi ſono egualmente buoni. In generale i più vecchi, e che da molto tempo ſono ſtati guardati dal grand' umido rieſcono incomparabilmente meglio che quelli tratti di freſco dalla cava, o ſtati eſpoſti lungamente all' ingiurie dell' aria; onde queſt,

ſolamente han biſogno dell' efficcamento nella ſtufa. Ma oltre di cio avvi ancora notabiliſſima differenza tra una ſpecie e l'altra di marmo: ne ho trovato di tali, che ſenza riſcaldarli ne tampoco aſciugarli fanno ſempre a meraviglia, e di tali altri, che anche con una tal preparazione non corriſpondono troppo bene; a meno che non ſi continui loro il' caldo durante il tempo dell' eſperienze. Sopra tutti finora ho trovato eccellente il bel marmo bianco di Carrara. Ciò non pertanto io non ſo abbaſtanza raccomandare di riſcaldare e queſto, e gl' altri marmi, almeno un poco innanzi adoperarli: con che vantaggian ſempre per eccellenti che ſiano ed eſſendo cattivi vengono a migliorarſi inſignemente, e ſi adagguagliarſi ai più buoni: anzi poſſo dire, per eſperienza che la maggior parte dei marmi di lor natura poco buoni, ove ſiano ben riſcaldati previamente, e in ſeguito ſi mantengano tiepidi tutto il tempo dell' eſperienza, prevalgono ſe non a tutti a molti dei migliori non punto riſcaldati.

16. A chi però ſembraſſe incomoda queſta preparazione (la quale per altro a che ſi riduce? Ad eſporre il piatto di marmo al ſole, od a preſentarlo per poco d'ore innanzi al fuoco d'un cammino, o al più tenerlo ſu d' uno ſcaldavivande ove ſia o cener calda o pochi carboni accesi) io ſuggerirò il mezzo di diſpenſarſene: baſta di dare alla faccia piana del marmo una buona mano di vernice copal, da aſciugarſi quindi in una ſtufa ben calda o in un forno tantoche prenda un color d'ambra tirante al bruno. La vernice medeſima d'ambra ſarà ottima, ſiccome pure la lacca. Con ciò non ſolo i buoni, ma i cattivi marmi eziandio ſerviranno mirabilmente all' intento (chè ſi pure un gran vantaggio) e ſerviranno in ogni tempo ſenza previo riſcaldamento, o almeno ſenza continuarlo loro durante l'eſperienza.

17.

17. Appigliandosi a questo spediente della vernice si può benissimo in luogo del piatto di marmo far servire una lamina di metallo eguale all' altra lamina o sia scudo, e resa perfettamente combaciante col lavorare, come si è detto (14.), i due piani un sopra l'altro. Se la vernice si desse ad amendue le faccie combacianti, non sarebbe male; ma basterà anche il darla all' una o all' altra: in questo caso però una mano sola di vernice, che saria più che sufficiente, per la lastra di marmo, forse non basteria per la lamina metallica, ma ce ne vorrebbe una seconda ed anche una terza mano.

18. Ma con ciò, mi si dirà, noi siam ricondotti ad un vero piatto d'elettroforo, giacchè l'intonaco di vernice tien qui luogo del sottile strato di resina. Io non voglio negarlo; anzi dirò, d'aver provato che e il metallo, e il marmo, singolarmente così invernicati, son tali, che l'elettricità vi si affigge facilmente, e non men facilmente vi si eccita per istrofinamento, talchè il solo strisciare che faccia lo scudo sulla superficie inverniciata del piatto, o il percuoterla con qualche forza mentre si viene a posar sopra cotesto scudo, basta perchè poi dia segni sensibili di elettricità allorchè si distacca. Talora anzi non è possibile d'impedire che questo succeda, per quanto si procuri di posar lo scudo pian piano, e di alzarlo senza punto strofinare. Tal importuna elettricità però è debolissima e non si suscita che nel caso in cui il piatto verniciato si trova asciugatissimo, e ancor tiepido dal sole o dal fuoco. Si fatto asciugamento e riscaldamento adunque non solo non è necessario per le nostre sperienze quando adoperiamo un piano verniciato, com' è necessario quasi sempre ove s'adoperi il marmo nudo (13. 15. 16.), ma non è neppure molto proficuo da una parte; e dall' altra egli è assolutamente pregiudizievole, per ciò che dando luogo ai fenomeni d'*elettroforo*, può facilmente produrre equivoci ed incertezze (8.).

19. Qual vantaggio adunque, mi ſi dirà un' altra volta, nell' adoperare in luogo di un ordinario elettroforo, un piatto inverniciato? Altronde ſi è pur detto che vuol preferirſi un piatto nudo di marmo (11. e ſeq.). Il vantaggio del piatto verniciato ſopra l' un ordinario d'elettroforo è 1° che la vernice ſarà ſempre più ſottile di qualunque incroſtatura reſinoſa; 2°. che quella meglio che queſta puo laſciare la ſuperficie del piatto, ſia di marmo ſia di metallo, piana e liſcia in modo, che lo ſcudo vi s'adatti ancora quaſi a coeſione: due circoſtanze, quali veduto già abbiamo (6. 7. 14.) quanto influiſcano alla buona riuſcita delle ſperienze di cui ſi tratta. Riguardo al piatto nudo di marmo, egli é ben vero che queſto puo ſervire egualmente bene, e forſe meglio s'egli é d'ottima qualità, o allorche ſi tenga convenevolmente riſcaldato (15.); ma valutando bene le coſe, l'incomodo, cioè di tal preparazione, qualunque egli ſia (16.), e la difficoltà d'aver il marmo perfetto (15.), credo che convenga ancora l'eſpediente della vernice, che vi diſpenſa da tutto queſto (16.). Vi reſta è vero l'altro inconveniente di poteryiſi per poco affiggere l'elettricità; ma oltrecchè anche il marmo perfettamente aſciutto, e molto più ſe caldo, non va eſente da tal incomoda diſpoſizione, egli non è poi tanto difficile di ciò ſcanſare adoperando le debite attenzioni; e l'accurato ſperimentatore non laſcerà di aſſicurarſi coi mezzi che giá ſi ſono indicati (8.) che non trovaſi neppur ombra di elettricità impreſſa alla faccia verniciata, quando imprende a fare col condenſatore delle ſperienze delicate.

20. Al piatto di marmo e di metallo inverniciato va di paro un piano qualunque coperto di buona tela incerata ſecca e monda, di taffetá cerato, di raſo o d'altro drappo di ſeta il quale più che è ſottile è meglio: dico, che queſti piani coſi veſtiti van di parò agl' altri verniciati, ſtante che non han biſogno che

d'avere

d'avere cotal veste ben asciuta, e al più un pocolino riscaldata prima di servirsene; anzi pure e la tela, e il taffetà, incerati non attraendo molto l'umido, non hanno d'ordinario neppur bisogno d'essere posti al sole o al fuoco innanzi farne uso. Il ciamberlotto, il feltro, ed altri drappi di pelo son buoni anch' essi, ma men della seta; quei di lana, o di cotone, meno ancora; e i più infelici sono quei di canape e di lino: ad ogni modo un buon asciugamento, e un gentil calore continuato possono abilitare anche questi, siccome pure abilitano la carta, il cuojo, il legno, l'avorio, e gl' altri ossi, tutti insomma i corpi che sono da sestessi imperfettissimi conduttori, anzi non conduttori, ma troppo bibaci dell' umido, cui perciò convien espellere fino a un certo segno.

21. Dico *fino a un certo segno:* perchè un troppo grande isolamento è pregiudizievole anzichè no, come si è già accennato (6. 12.), e come si farà più chiaramente vedere nell 2ª parte di questa memoria. Or dunque se i detti corpi vengano spogliati affatto d'umido, posti per esempio a seccare nel forno, in tal caso siccome diverranno veri e perfetti coibenti a par delle resine; così non serviranno più al nostro intento, a men che non sian ridotti ad uno strato sottile, e questo strato applicato ad un conduttore (ivi) in modo che ne risulti un vero piatto d'elettroforo.

22. Non lascerò da ultimo di dire, che si puo rendere l'apparecchio ancora più semplice, se si applichi sia l'intonaco di vernice, sia la veste d'incerato, sia il taffetà od altro velo di sata, sia infine qualunque materia semicoibente, alla faccia inferiore dello scudo, in luogo di coprirne il *piatto*; il quale in questo caso diventa inutile, servendo allora in sua vece un piano qualunque egli sia, un tavolo di legno o di marmo, anche non ben asciutti, una lastra di metallo, un libro, od altro conduttore, buono o

cattivo che ſia, ſol che vi ſi poſſa applicare convenientemente la faccia veſtita dello ſcudo.

E' in vero altro più non ſi ricerca per la buona riuſcita delle ſperienze, ſe non che l'elettricità, che tende a paſſare dall' uno all' altro dei piani combaciantiſi, incontri ſull' una delle ſuperficie tale reſiſtenza, che valga a trattenerla, come ſi è già accennato (11), e ſi farà chiaro nella ſteſſa ſeconda parte; dove al dippiù moſtreraſſi, come a tal effetto baſti anche una picciola reſiſtenza. Ciò poſto, che lo ſtrato ſottile coibente o quaſi coibente tenga al piano di ſotto, o a quel di ſopra, egli è lo ſteſſo: quello che importa è che ſi combacino bene (7); la qual coſa non è ſi facile ottenere allorchè ſi poſa lo ſcudo ſu d'un tavolo, od altro piano non preparato a bella poſta. Egli é ſolo per queſta ragione, per ottenere cioè un più eſatto combaciamento, che io dò la preferenza a due piani lavorati un ſopra l'altro intonacandoli quindi od amendue, o uno ſolo, qual più mi piace (14. 17.). Del reſto la comodità d'avere per tutto l'apparato una ſola lamina di metallo inverniciata da un lato, o coperta di taffetà, e dall' altro guernita di trè cordoncini di ſeta, fa che io me ne ſerva più comunemente: e la riuſcita ſe non agguaglia per avventura quella dell' altro apparechio compoſto dei due piani lavorati un ſupra l'altro, è tale però che baſta d'ordinario all' intento.

23. Fin qui noi abbiamo conſiderato l'utile che ſi può ritrarre dal noſtro apparecchio condenſatore applicato ai conduttori per eſplorare l'elettricità atmoſferica, allorchè è debole affatto ed impercettibile *. Queſto però, a cui vien deſtinato prin-

* A queſto propoſito non debbo ommettere, che ne' pochi giorni in cui m'applicai a ſpiare l'elettricità atmoſferica col ſoccorſo del *condenſatore*, non ſon rimaſto ſenza buon frutto raccorne. Il Sig. CANTON, ed altri aſſicuravano di aver ottenuto dall'

principalmente, non è il ſolo uſo che far ſe ne poſſa, nè il ſolo vantaggio che ci procura: ſerve altreſi molto per l'elettricità artificiale, a diſcoprirla cioè ove per altra via non ſi manifeſterebbe, o a renderne i ſegni aſſai più coſpicui. Molti ſono i caſi, in cui, l'elettricità, che è nulla in apparenza, o molto dubbia, vi ſi renderà chiara e ſenſibiliſſima coll' ajuto di tal apparecchio: ne andrò accennando per modo d'eſempio alcuni.

24. 1°. Una boccia di Leyden caricata, e quindi addotta alla ſcarica coll' applicarvi tre, o quattro volte l'arco conduttore, e con replicati toccamenti della mano, vi ſembrà omai ſpogliata affatto della ſua elettricità. Ma che? Toccate coll' uncino di tal boccetta la lamina metallica poſata convenevolmente (cioè ſopra qualunque piano, s'ella é ben inverniciata nella faccia inferiore, o veſtita di taffetà, &c. oppur s'è nuda ſopra ſottile ſtrato reſinoſo, o ſu d'un incerato, o ſu drappo di ſeta, o ſopra tavolo di legno inverniciato, o ſopra laſtra di marmo ben aſciutto) e toſto alzata cotal lamina o ſcudo ne avrete ſegni elettrici ſenſibiliſſimi: dal che concluderete che l'elettricità della boccetta non era già tutta ſpenta, come appariva. Che ſe queſta aveſſe una carica ſenſibile a ſegno di

dall' apparato atmoſferico de ſegni elettrici più vivi dell' ordinario in tempo di qualche aurora boreale; ma molti de fiſici non ſono perſuaſi ancora che l'elettricità influiſca in queſte meteore, e alcuni lo negano formalmente. Io ſteſſo ne dubitai moltiſſimo: ora però parmi la coſa certa; e poſſo dire d'aver veduto e toccato con mano. In quella belliſſima aurora comparſa nella notte dei 28 ai 29 Luglio dell' anno 1780. quando ſalendo a poco a poco dall' orizzonte fu aſceſa tra la 4. e le 5. italiane allo zenit, ſpargendo tutt' all' intorno un vaghiſſimo lume roſſigno, il cielo altronde eſſendo ſereno e venſoſo, ſi ottennero col favore dell' apparechio condenſatore da un conduttore atmoſferico ordinario molte belle ſcintillette chiare e crepitanti: quando in tutti gl' altri tempi ſereni, e in ogni ora del giorno e della notte dall' iſteſſo conduttore, e coll' ajuto dell' iſteſſo condenſatore o non ottienſi ſcintilla o minutiſſima ſoltanto; e ciò perchè quel conduttore atmoſferico non è nè alto molto, nè molto ben ſituato.

attrarre

attrarre un legger filo, in tal caſo lo ſcudo toccato dall' uncino anche per un ſol momento, e quindi alzato vibrerà vivace ſcintilla. Ripoſto quello, e ritoccato coll' iſteſſo uncino della boccia, e rialzato di nuovo, ne otterrete una ſeconda ſcintilla, nulla o poco men vivace della prima; e un tal giuoco potraſſi continuare per molte volte con pari diletto e meraviglia.

Coteſto artificio di produr ſcintilla, e replicate, con una boccetta, che non ha carica ſufficiente per farlo da ſê ſola, vi appreſta una grande comodità per varie ſperienze dilettevoli, come quelle della mia piſtola, e della lucerna ad aria infiammabile, maſſimemente trovandovi provveduto d'una di quelle boccette preparate alla maniera del Sig. TIBERIO CAVALLO *, le quali ſi poſſono portare cariche in taſca molto tempo. Queſte poichè conſervano una carica ſenſibile alcuni giorni, ne conſerveranno una inſenſibile per ſettimane, e meſi: inſenſibile, dico, ſenza l'ajuto del noſtro apparecchio condenſatore; ma con queſto ſenſibiliſſima, e più che ſufficiente all' uopo di accender la piſtola, &c.

25. 2°. Avete una macchina elettrica meſchina, così mal in ordine, e in tali circoſtanze sfavorevoli d'umido &c. che non potete trarre la più piccola ſcintilla dal conduttore, il quale appena attrae un leggeriſſimo filo, o non giugne neppur a tanto. Or via fate toccare a tal conduttore inerte il noſtro apparecchio, oſſia lo ſcudo poſato come conviene, e laſciate che il toccamento duri per qualche minuto, tenendo ſempre in azione la macchina, e vi riuſcirà di ottenere col ſolito giuoco di ſtaccare lo ſcudo dal ſottopoſto piano, una buona ſcintilla, e ogn' altro ſegno vivace.

26. 3°. Sia pur la macchina buona, e agiſca a dovere; ma il conduttore troviſi così male iſolato, che l'elettricità non vi ſi

* Vedi il ſuo trattato di elettricità.

poſſa

possa accumulare a segno di dar scintilla, e neppure di attrarre un filo; come quando l'istesso conduttore tocca al muro della stanza, o quando una catena pende da esso sopra un tavolo, e fin sopra il pavimento della stanza. In simil caso crederete che l'elettricità per quelle comunicazioni si disperda intieramente, ma cercando più oltre, ricorrendo cioè al condensatore, troverete che un poco se ne trattiene ad ogni momento nel conduttorre tuttochè non isolato, e tanto che durando l'azione della macchina qualche tempo, i molti pochi raccolti insieme nello scudo, per la vantaggiosa disposizione ch' egli ha di tirar sopra di se l'elettricità (2.) fanno ch' il medesimo sia poi in istato di dar segni abbastanza forti.

27. 4°. L'ordinaria maniera di strofinare alcuni corpi, e quindi presentarli ad un elettrometro, onde vedere se per tal mezzo abbiano o no contratto qualche elettricità, è in molti casi insufficiente, di modo che sovente si crede che sia nulla, sol perchè debolissima. Si trae dunque un gran vantaggio strofinando corpi dubbi collo scudo o lamina metallica del nostro apparecchio, che in questo caso deve esser nuda, poi levatala in alto isolata interrogando lei medesima, la quale darà segni abbastanza sensibili per qualunque picciola ad insensibile elettricità eccitata nel corpo, contro cui si è strofinata, e dinoterà quale specie di elettricità quello abbia contratta, giacchè si sa che debbe essere nei due contraria. Anche il Sig. CAVALLO si serviva di questo mezzo per iscoprire l'elettricità in molti corpi *. Ma ve n' è uno a certi riguardi migliore, che certamente nè egli nè altri, ch' io sappia, han conosciuto. Quando il corpo, di cui si vuol provare la virtù, non è tale che vi si possa addattare in piano la lamina metallica per dimenarla sopra strofinando, si puo fare così: posata la lamina sopra il solito piano

* Vedi il suo trattato, cap. VI. p. iv.

semicoibente, si strofini essa, o meglio si percota a vari colpi col corpo in questione; il che fatto si levi la lamina, e si osservi se è elettrizzata: lo sarà certo nel caso che vi siate servito a percuoterla di una striscia di cuojo, d'una corda, d'un pezzo di panno, di feltro, o simili cattivi conduttori; e lo sarà assai più che se l'aveste sferzata o strofinata par egual maniera coi medesimi corpi stando essa lamina metallica isolata. In somma coll' uno o coll' altro degl' indicati mezzi voi otterrete elettricità da corpi che non avreste mai creduto che godessero di questa virtù, anche da corpi non secchi, da tutti infine eccetto solo i metalli e i carboni: dirò dippiù, ch' io ne ho ottenuto qualche volta strofinando la lamina metallica colla mano nuda.

28. 5°. Si è cercato se il calore, l'evaporazione, le fermentazioni, &c. producano qualche grado di elettricità, ossia cagionino qualche alterazione alla dose naturale del fluido elettrico nei corpi che subiscono cotesta azione, e in quelli che ne sono in contatto. La ricerca era di grande importanza per fissar pure qualche idea sull' origine dell' elettricità naturale, ossia atmosferica. Io so di molti che hanno tentato specialmente sull' evaporazione delle sperienze invano, anche hanno infine rinunciato alla speranza di ottenere per tal mezzo segni elettrici; nè so d'alcuno che sia ancor giunto ad ottenerli. Le mie proprie sperienze non avean avuto miglior successo; ad ogni modo ben lungi di rinunciare ad ogni speranza, le andava sempre più nodrendo. Da gran tempo io aveva imaginato che le dissoluzioni, le effervescenze, le volatilizzazioni, &c. sconvolgendo le minime particelle, e forma e posizione mutandone, doveano coll' alterazione delle forze mutue di esse particelle aumentare o diminuire le rispettive capacità dei corpi sottoposti a que moti intestini, e conseguentemente occasionare dove condensazione, dove rarefazione del fluido elettrico: ne era così persuaso, che

non

non sapevo darmi pace che l'elettricità non si manifestasse per alcuno di tai processi; di tal mancanza di segni pertanto io ne accagionavo parte alla debolezza dell' elettricità che per tal modo si eccitava, parte alla dissipazione di essa prodotta dai vapori medesimi che si sollevano durante il processo, e distruggono quasi intieramente l'isolamento: mi compiaceva però sempre a pensare, che l'avrei un giorno potuta scoprire cotesta elettricità fugace, moltiplicando le sperienze, e mettendovi più di attenzione e di accuratezza *. Due anni sono allorchè fui passo passo condotto alla maniera di condensare a un segno si grande l'elettricità coll' apparecchio qui descritto, i miei pensieri si rivolsero nuovamente all' oggetto delle antiche mie ricerche, e concepj molto più fondata speranza di poter iscoprire qualche cosa, e già mi proponeva di applicarmi, a tali sperienze, quasi presagendo la riuscita; ma varj accidenti le ritardarono fino al Marzo e Aprile di quest' anno, in cui intrapresi avendole a Parigi in compagnia di alcuni membri dell' Accademia R. delle Scienze, mi riuscì finalmente di ottenere segni non dubbj di elettricità (che dico segni non dubbj?) fin la scintilla elettrica dall' evaporazione dell' acqua, dalla semplice combustione dei carboni, e da varie effervescenze, come quelle che producono l'aria infiammabile, l'aria fissa e l'aria nitrosa.

29. Terminerò la prima parte di questa memoria coll' dire, che oltre gli accennati vantaggi, ed altri del medesimo genere, che ne procura il nostro condensatore considerato semplicemente come istromento, atto ad ingrandire i segni dell' elettricità; le varie sperienze che possono farsi con esso spargono eziandio molto

* Tutti questi miei pensieri sono esposti in una dissertazione latina stampata l'anno 1769, che ha per titolo, *De vi attractiva ignis electrici, ac phenomenis inde pendentibus, ad* JOHANNEM BAPTISTAM BECCARIAM, &c.

lume ſulla teoria elettrica, per quella parte maſſimamente che riguarda l'azione delle atmosfere: lo che andiamo a vedere nella parte 2ª.

PARTE SECONDA.

IN qual maniera un conduttore accoſtandoſi a un altro ſotto certe condizione, ſi truovi in iſtato di ricevere una ſtraordinaria quantità di elettricità.

30. Le ſperienze riportate nella prima parte di queſta memoria ci hanno abbaſtanza moſtrato come una lamina metallica o qualſivoglia piano conduttore, cui ſoglio appellare *ſcudo*, applicato ad un altro piano, il quale opponga, o per la qualità ſua di cattivo conduttore, o per l'interpoſizione di un ſottile ſtrato coibente, una certa non grande reſiſtenza alla trasfuſione dell' elettricità, come diſſi, tale ſcudo in ſiffatta poſizione atto ſia a tirare ſopra di ſè e raccorre nel ſuo ſeno maggiore copia di elettricità, che ſe ſi trovaſſe in qualſivoglia modo perfettamente iſolato. Abbiam veduto come facendolo toccare all' uncino di una boccia di Leyden, al conduttore di una macchina elettrica, o a quello dell' elettricità atmosferica, infine a qualunque potenza o ſorgente elettrica, anche quando l'elettricità è deboliſſima e affatto impercettibile, pur gli ſe ne comunica tanto da poter manifeſtarſi quindi con ſegni molto vivaci, toſto che ſi leva eſſo ſcudo in alto. Or qui intraprendiamo di ſpiegare un tal fenomeno: e la ſpiegazione medeſima ſervirà più ch' altra coſa a facilitare la pratica delle ſperienze di queſto genere.

31.

31. Adunque il tutto ſi riduce a queſto: che la lamina o ſcudo ha molto e molto maggiore capacità nel 1° caſo, quando cioè poſa ſul piano avente le condizioni indicate (præc. e 11. 12. 22.), che nel 2°, in cui tienſi eſ. gr. in alto ſoſpeſo per i ſuoi cordoncini di ſeta, o per un manico iſolante, oppur che poſa ſopra un groſſo ſtrato coibente, o ſopra un piatto iſolato.

Per dilucidare queſto punto eſſenziale, prendiam le coſe da più lontano.

32. Non vi vuol molto a comprendere, che' ivi è maggiore capacità, dove una data quantità di elettricità ſorge a minor intenſità, o che è lo ſteſſo, quanto maggior doſe di elettricità è richieſta a portare l'azione a un dato grado d'intenſità; e *vice-verſa*: a dir breve, la capacità e azione, o tenſione elettrica ſono in ragione inverſa.

Farò qui oſſervare ſul principio, ch' io dinoto col termine di tenſione (che volentieri ſoſtituiſco a quello d'intenſità) lo ſforzo che fa ciaſcun punto del corpo elettrizzato per disfarſi della ſua elettricità, e communicarla ad altri corpi: al quale ſforzo corriſpondono generalmente in energia i ſegni di attrazione, ripulſione, &c. e particolarmente il grado a cui vien teſo l'elettrometro.

33. Ciò che abbiam detto comprenderſi facilmente che la tenſione debb' eſſere in ragione inverſa delle capacità, ci viene poi moſtrato nella maniera più chiara dall' eſperienza. Siano due verghe metalliche, una lunga 1 piede e l'altra 5. di groſſezza equali. S'infonda alla prima tanto di elettricità, che giunga a vibrare un elettrometro anneſſo a 60 gradi: ſe in queſto ſtato ſi farà toccare quella all' altra verga, l'elettricità compartendoſi equabilmente ad ambedue, diminuirà di tenſione tanto appunto, quanto la capacità ſi truova ora accreſciuta, cioè 6 volte: locchè ci farà vedere l'elettrometro, ſmontando dai 60, ai

10 gradi. Così ſe l'iſteſſa quantità di elettricità veniſſe a diffonderſi in un conduttore 60 volte più capace, non rimarrebbe che $\frac{1}{60}$ della primiera tenſione, cioè un grado ſolo: come *viceverſa* la tenſione di 1 ſol grado di cotesto gran conduttore, o d'altro qualunque, ſalirebbe a 60, ove la di lui elettricità veniſſe a raccorſi e condenſarſi in una capacità 60 volte minore.

34. Or non ſolo conduttori di mole e maſſa diverſi hanno diverſa capacità; ma anche l'iſteſſo conduttore puo averne una maggiore o minore, ſecondo varie circoſtanze; alcune delle quali non ſono per anco ſtate conſiderate, come ſi conviene. E ſtato oſſervato che l'iſteſſo conduttore acquiſta o perde in capacità, a miſura che ſi aggrandiſce, o ſi riſtringe di ſuperficie; ſecondo che una catena metallica ef. gr. ſi diſpiega in lungo, o ſi ammucchia, ſecondo che vari cilindri contenuti un nell' altro, come quelli d'un cannochiale ſi traggon fuori, o ſi fanno rientrare, &c. Quindi ſi è concluſo generalmente che la capacità non è in ragion della maſſa, ma bene in ragion della ſuperficie del conduttore: come FRANKLIN ha dimoſtrato appunto coll' indicato ſperimento della catena.

35. Queſta concluſione e giuſta, ma non comprende ancor tutto, perocchè anche con ſuperficie egualmente grandi ſi ha maggiore o minore capacità, ſe ſiano i conduttori diverſamente conformati. Eſſa ſi troverà maggiore di molto in quel conduttore che avrà più lunghezza comunque ſia d'altrettanto men groſſo, coſicchè la quantità della ſuperficie rimanga eguale: come WATSON ed altri aveano già oſſervato, e come io mi luſingo d'aver poſto in miglior lume nella mia memoria ſulla capacità de conduttori ſemplici *, nella quale dimoſtro il grande vantaggio di un conduttore coſtrutto di molte verghe di legno coperte di foglia metallica, e collocate in lungo punta a punta,

* Fu pubblicata queſta memoria in un opera periodica di Milano intitolata *Opuſcoli Scelti*, per l'anno 1778, e nel Giornale dell' Ab. ROZIER l'anno ſequente.

ſopra

sopra gl' ordinarj conduttori assai più grossi e meno lunghi. Se l'istesso conduttore colla grossezza e lunghezza medesima non sia diritto, ma assai curvo, e molto più se essendo es. gr. un fil di ferro, abbia molti torcimenti, o si ripieghi indietro, avrà minore capacità; così pure l'avranno minore le indicate verghette, se invece d'esser collocate punta a punta in linea retta, lo siano ad angolo, e peggio se s'accostino parallele.

Le sperienze ed osservazioni da me rapportate in quello scritto, ed infinite altre, massimamente quelle intorno al così detto *pozzo elettrico*, concorrono tutte a provare, che la capacità è in raggione non delle superficie qualunque esse sieno, ma delle *superficie libere dall' azione delle atmosfere omologhe:* nella quale rettificata proposizione converranno tutti quelli, che si faranno a considerare i principali fenomeni delle atmosfere elettriche.

36. Ma v' è dippiù ancora: e questo è propriamente che fa al nostro caso. L'istesso conduttore ritenendo la stessa superficie, e la forma sua non mutata, acquista maggiore capacità allorachè in luogo di rimanere isolato solitariamente, si affaccia a un altro conduttore non isolato, e l'acquista tanto sempre maggiore quanto vi si affaccia più davvicino, e quanto le superficie che si presentano un l'altro sono più larghe. Io chiamo quel conduttore isolato che ne ha un altro di fronte (sia questo non isolato, come nel caso nostro, sia anche isolato, elettrizzato o no), lo chiamo *conduttore conjugato*; e già io aveva promesso nella mentovata dissertazione, trattato avendo della capacità dè *conduttori semplici*, o *solitari*, di trattare in seguito di quella de *conduttori conjugati*.

37. Tale circostanza, che accresce prodigiosamente la naturale capacità di un conduttore, quella è sopra tutto, a cui non truovo che si sia fatta ancora la debita attenzione; molto meno che alcuno ne abbia tratto quei vantaggi, che dall' applicazione facilmente

facilmente ne derivano. Ma veniamo a quelle ſperienze più ſemplici, che ci mettono ſott' occhio queſta accreſciuta capacità.

Prendo un diſco di metallo, il ſolito ſcudo d'elettroforo per eſempio, e tenendolo in alto iſolato lo elettrizzo a una data forza, quanto baſta, ſupponiamo, a fare che un elettrometro anneſſo ſi tenda a 60 gradi; calando indi eſſo diſco gradatamente verſo un tavolo od altro piano deferente, ecco che decade l'elettrometro a 50, 40, 30 gradi. Non crediate perciò che ſia ſcemata a queſto punto la quantità d'elettricità che il diſco poſſiede, la quale anzi, purchè quello non ſia giunto a tale vicinanza dell' altro piano deferente da dar luogo alla trasfuſione collo ſcoccare di qualche ſcintilla, ſi ſarà mantenuta nell' intierezza ſua, quanto almeno la lunghezza del tempo, lo ſtato dell' aria e dell' iſolamento lo permette. Onde dunque tale e tanto abbaſſamento di *tenſione?* Non altronde che dall' accreſciuta capacità del diſco, or non più ſolitario, ma *conjugato.* In prova di che ſe ſi ſollevi di nuovo gradatamente, riſalirà il ſuo elettrometro a 40, 50, e fin preſſo ai 60 gradi di prima (riſalirebbe a 60 giuſto, ſe ſi poteſſe impedire affatto il diſſipamento nell' aria, e lungo gl' iſolatori non mai perfetti abbaſtanza); a miſura cioè che allontanandoſi dall' altro piano deferente ritorna il diſco a quella più anguſta capacità, che gli compete quand' è ſolitario.

38. La ragione di un tal fenomeno ſi deduce facilmente dall' azione delle *atmoſfere elettriche.* Quella del diſco, che or ſuppongo elettrico *per eccesſo* ſi fa ſentire al tavolo, od altro qualſivoglia conduttore, a cui ſi affaccia in guiſa che il fuoco di queſto, giuſta le note leggi, ritirandoſi ſi dirada nelle parti più vicine al diſco ſovraſtante, e tanto più ſi dirada, quanto eſſo diſco elettrico ſi va più accoſtando. Se l'elettricità di queſto è *per diffetto*, il fuoco del tavolo o piano inferiore qualunque ſia, accorre

accorre e ſi addenſa verſo la ſuperficie medeſima, che guarda il diſco, e che ne ſente più davvicino l'azione. Inſomma le parti immerſe nella sfera di attività del diſco contraggono un elettricità *contraria*, elettricità che puo dirſi *accidentale*, e che portando in certo modo un *compenſo* a quella *reale* del diſco medeſimo, ne diminuiſce la *tenſione*, come appunto ci dimoſtra l'abbaſſamento dell' elettrometro (præc.)

39. Due altre ſperienze porranno in maggior lume queſta azione reciproca delle atmosfere elettriche, mercè di cui ora s'infievoliſcono, ora ſi rinforzano mutuamente le *tenſioni* oſſia azioni elettriche di due corpi col ſolo avvicinarſi l'uno all' altro, ritenendo ciaſcuno nè più nè meno la ſua doſe di elettricità.

Cominciamo da quelle che ſi rinforzano. Queſte ſono le *atmosfere omologhe*. Siano pertanto due piani conduttori, due diſchi, elettrizzati o per eccesſo amendue, o amendue per difetto. Si affaccino queſti, e ſi vadano gradatamente avvicinando: vedraſſi che influiſcono l'uno ſull' altro in modo, che la *tenſione* elettrica s'accreſce in amendue a proporzione del più grande avvicinamento, e della quantità di ſuperficie che ſi preſentano: ciò, dico, vedraſſi dal maggiore innalzamento de riſpettivi elettrometri, e dalla ſcintilla, che eſplorando l'uno o l'altro di quei diſchi ſcoccherà a maggior diſtanza, che ſe ciaſcuno foſſe rimaſto con tutta la ſua elettricità *ſolitario*. In quello ſtato adunque di avvicinamento egli è chiaro, che ciaſcuno de due conduttori *conjugati* ha una minore capacità; giacchè a proporzione che ſono già *attuati* a un più altro grado di elettricità, lor reſta meno per giugnere al ſommo, o a parlar più giuſto, maggiore è la reſiſtenza che oppongono ad un ulteriore carica, conformemente a quanto oſſervato già abbiamo (33.) che la tenſione eſprime lo ſforzo, onde un corpo tende a disfarſi dell' elettricità, e a comunicarla altrui. Così una boccia di

Leyden carica a un grado un poco maggiore di quello dei diſchi *ſolitari*, la quale per conſeguenza *darebbe* loro in tale ſtato, *riceverà* all' incontro da eſſi quando eſſendo *conjugati* vi prevale la *tenſione*: ritornando queſti ſolitari, cederanno un altra volta alla boccetta, &c.

Or anche ſi comprende quello, che abbiamo fatto più ſopra oſſervare (36.), onde ſia cioè che un filo metallico ripiegato, o molte verghe poſte allato e vicine le une alle altre, abbiano minore capacità che diſpoſte l'une all' altre in una linea retta; perchè con ſuperficie equali un conduttore corto e groſſo abbia meno capacità d'un lungo e ſottile; perchè infine la capacità ſia in ragione delle ſuperficie *libere*, o meno *attuate* dall' influſſo delle *atmoſfere omologhe*.

40. Siano ora i medeſimi diſchi della ſperienza precedente ambi ellettrizzati, ma uno *per eccesſo* l'altro *per difetto*, ben ſi vede che ne ſeguiranno effetti contrarj: cioè l'influenza vicendevole delle atmoſfere, per cui l'uno è *attuato* dall' altro, produrrà un *compenſo* od *equilibrio accidentale*, onde diminuiraſſi la *tenſione* in amendue, cadrà l'elettrometro, &c. Allora io dico che trovaſi accreſciuta in ciaſcuno de due diſchi la capacità, inquantochè opporrà ciaſcuno minor reſiſtenza ad un ulteriore carica dell' elettricità che già poſſiede, e gliene rimarrà dippiù a prendere per giugnere a un dato grado di *tenſione*. Così una boccetta di Leyden carica dell' iſteſſa ſpecie d' elettricità d'uno di queſti diſchi, e all' iſteſſo grado ed anche al diſotto, potrebbe tuttavia aggiungere all' elettricità di quello, quando, trovandoſi *conjugato*, la ſua *tenſione* è indebolita dall' atmoſfera elettrica contraria del diſco compagno; ma rimoſſo quello da queſto, e divenuta in lui le *tenſione prevalente*, darebbe egli della ſua elettricità alla boccetta, &c.

41. Non reſta più ora che fare un applicazione di queſt' ultima ſperienza a quelle riportate di ſopra (38.), in cui il diſco

elettrizzato si affaccia a un piano conduttore non isolato. S'egli è vero, come supposto abbiamo che questo nella parte più vicina a detto disco elettrico, per l'azione della di lui atmosfera, si compone ad un elettricità contraria, vale a dire che il fuoco ivi si dirada qualor l'incombente elettricità sia *in più*, o vi si condensa qualor sia *in meno* (39.), dovrà dunque nascere l'istesso *equilibrio* accidentale, l'istesso *compenso*, e alleviamento alla *tensione* elettrica del disco, lo stesso abbatimento dell' elettrometro, come appunto si osserva (38.): quindi l'accresciuta capacità di esso disco; quindi la maggior dose di elettricità che potrà ricevere (prec.) &c.

42. La cosa è già bastantemente chiara, ma si renderà ancora più manifesta, e toccherassi con mano, se si venga ad isolare il piano conduttore (supponiam che questo sia parimenti un disco metallico, che chiameremo disco inferiore) affacciato già al disco elettrico, e dopo si allontanino un dall' altro; giacchè allora compariranno realmente in esso piano o disco inferiore i segni dell' elettricità contraria da esso lui acquistata allorchè non era isolato, e trovavasi immerso nell' atmosfera del disco superiore. Cotesto disco superiore poi, il quale intantochè si allontana, ricupera la *tensione*, che l'avvicinamento gli avea fatto perdere, la perderà di nuovo a misura che si accosterà un altra volta al disco inferiore, e la farà perdere a lui medesimo, in virtù dell' azione reciproca delle contrarie elettricità (41.) a indicare le quali vicende è opportuno che trovisi un elettrometro annesso a ciascuno de dischi; poichè il linguaggio dell' elettrometro è il più significante di tutti, e ardisco dire ch' esso solo vi dà la spiegazione di tutti i fenomeni riportati in questo scritto, e d'infiniti altri analoghi.

43. Che se il disco inferiore si truovi isolato, al primo affacciarvi il disco superiore elettrizzato, e isolato rimanga tutto il tempo che questo vi sta sopra, in tal caso venendo *attuato* dalla

di lui atmosfera, acquisterà quella che chiamo elettricità *omologa accidentale*, cioè una *tensione* od azione elettrica, con cui fa sforzo di consequire l'elettricità contraria; il che non venendogli dato di effettuare, per l'isolamento in cui si truova, non potrà neppur *compensare* nel dovuto modo l'elettricità del disco incombente, nè quindi diminuire in lui la *tensione* notabilmente, dimodochè l'elettrometro appena farà cenno di abbassarsi (il qual picciolo abbassamento si deve a quel poco di fuoco, che per l'azione dell' atmosfera elettrica può moversi nella spessezza del qualunque disco inferiore, o lungo i suoi sostegni isolanti non mai perfetti abbastanza); e per conseguenza non acquisterà il disco superiore maggiore capacità, onde poter prendere maggior dose di elettricità. Ma bene l'acquisterà, se un momento si venga a toccare il disco inferiore, onde distruggere in esso l'elettricità *accidentale omologa*, che vuol dire fargli prendere la *reale contraria*.

44. Se il disco inferiore non che trovarsi isolato, sia egli medesimo isolante, succederà lo stesso, cioè non potrà diminuire la *tensione* elettrica nè quindi aumentare la *capacità* del disco superiore accostatogli comunque. Non così però se cotal disco isolante sarà semplicemente un sottile strato che copra un conduttore; mercecchè questo piano conduttore che trovasi poco sotto, e in cui può moversi liberamente il fuoco, farà esso il giuoco di *compensare* l'elettricità del disco superiore; e lo strato isolante interposto diminuirà soltanto l'azion mutua delle atmosfere elettriche, in ragione della maggior distanza che pone tra l'uno e l' altro conduttore.

45. La *tensione* ossia azione elettrica del disco, la quale, come abiam veduto (38. 42.) va diminuendosi a misura ch' egli si affaccia più davvicino ad un piano deferente non isolato, è portata

tità a un tale decadimento quando si arriva quasi al contatto, il *compenso* od *equilibrio accidentale* essendo allora quasi perfetto, che dove l'elettrometro era teso a 60, 80, 100 gradi, si vedrà or disceso a 1 grado solo, ed anche meno. Quindi se il piano o disco inferiore opponga solo una picciola resistenza al trapasso dell' elettricità, o per l'interposizione d'un sottile strato coibente, o per la natura sua propria d'imperfetto conduttore, qual è il marmo asciutto, il legno secco, &c. tale picciola resistenza congiunta a quella della distanza comunque picciolissima non potrà essere superata da tale debolissima *tensione* del disco elettrico; il quale perciò non iscaglierà scintilla al piano (salvo che forse dagl' orli non ben ritondati, e nel caso che possieda una gran copia di elettricità); anzi conserverà tutta o quasi tutta la sua elettricità, dimodochè rialzandolo, il suo elettrometro ascenderà quasi al grado di prima. Più: potrà il disco senza gran detrimento della sua elettricità giugnere fino al contatto del piano imperfetto conduttore, e restarvi qualche tempo applicato: nel quale contatto la *tensione* elettrica trovandosi pressochè ridotta a nulla non ha forza di passare dal disco al piano che combacia se non lentissimamente.

46. Non andrà però così la bisogna, se ripetendo l'esperienza s'inclini il disco, e si porti a toccare il medesimo piano in costa: allora sussistendo in quello maggior *tensione* di elettricità (come ci mostrerà il fedele elettrometro), giacchè non vien bilanciata che corrispondentemente ai punti di superficie dell' uno che guardano davvicino la superficie dell' altro, cotal azione elettrica meno indebolita vincerà la piccola resistenza del marmo, o di qualsiasia altro imperfetto conduttore, e fino di un sottile strato coibente che trovisi interposto, cosicchè l'elettricità trasfonderassi realmente, e o s'affiggerà a cotesto strato coibente che copre il conduttore, o

paſſerà entro a queſto ſe è nudo fino a perderſi nel ſuolo *, e ciò in breviſſimo tempo: laddove vedemmo (præc), che non ne paſſa nulla o quaſi nulla in tempo aſſai più lungo, quando il contatto col medeſimo piano è il più ampio poſſibile. Il che ha l'aria di paradoſſo; ma pur ſi ſpiega così bene coi principj delle atmoſfere elettriche.

47. Quello che ſembra anche più paradoſſo, o almeno che ſorprende dippiù, ſi è che neppure il contatto di un dito, o di un pezzo di metallo comunicanti col ſuolo, replicato più volte e continuato per alcuni ſecondi, valga a ſpogliare intieramente

* Queſta ſpiegazione bene inteſa ci conduce a render ragione in generale della *virtù della punta*. A parlar giuſto una punta non iſolata, preſentata a un corpo elettrico non ha alcuna virtù propria per attirarne l'elettricità, ella ſi comporta ſemplicemente come un conduttore non iſolato che non oppone reſiſtenza al paſſagio del fluido elettrico. Se il medeſimo conduttore preſenta al corpo elettrico in vece della punta una palla, od una ſuperficie piana, non oppone già egli per queſto maggiore reſiſtenza; onde è dunque che l'elettricità non vi ſi getta egualmente all' iſteſſa diſtanza dal corpo elettrico? Ciò viene dall' indebolita *tenſione* oſſia azione elettrica di cotesto corpo in virtù della più larga ſuperficie preſentatagli da quel conduttore non iſolato, la quale ſuperficie componendoſi ad un elettricità contraria, offre maggior *compenſo* che una punta, come ſi è qui ſopra ſpiegato. Adunque in luogo di dimandare perchè una punta tragga o getti ſi da lungi l'elettricità, dovrebbeſi domandare piuttoſto perchè una palla o un piatto egualmente conduttore non lo faccino: allora io faro oſſervare che non è già un difetto di queſta palla o di queſto piano, come non è una virtù propria della punta che metta tale e tanta differenza; ma bene lo ſtato del corpo elettrico e della ſua atmoſfera (con cui intendo anche l'aria che lo circonda *attuata* ad una tenſione di elettricità omologa) il qual decade dalla ſua forte *tenſione* a proporzione che s'immergono in detta ſua atmoſfera e ſi affacciano a lui più punti di un conduttore non iſolato. Affievolita pertanto *l'azione elettrica*, è egli ſorprendente che non poſſa più ſuperare la reſiſtenza di quel lungo ſtrato d'aria interpoſta tra il corpo elettrico ed il conduttore, che ſupera agevolmente quando non preſentandogliſi alla medeſima diſtanza che una punta ſottile, *la tenſione* di eſſo corpo elettrico e dall' aria infinitamente meno bilanciata, ſuſſiſte nel ſuo pieno vigore?

dell'

dell' elettricità il disco posato sull' amico piano; ma ve ne lasci sovente tanto da poter dare ancora una scintilla quando in seguito si leva esso disco in alto. Invero tal fenomeno sarebbe inesplicabile anche nei nostri principj, se il dito o il metallo fossero perfetti conduttori, a segno di non opporre la minima resistenza al passagio del fluido elettrico, come si crede comunemente; ma la cosa non è così; e ce lo dimostrano queste stesse sperienze. I metalli dunque non sono che conduttori meno imperfetti degl' altri corpi. Ma, diraffi, noi vediamo che si trasfonde da un capo all' altro di un metallo, e da un metallo all' altro l'elettricità in un istante. Sia pure così di quell' elettricità che dispiega una forza sensibile a segno di tendere un elettrometro, o di attrarre un filo leggerissimo. Ma convien riflettere che al disotto di questo vi hanno da essere ancora altri gradi di elettricità impercettibili, i quali, dico io, non son valevoli a superare sì tosto quella qualunque piccola resistenza che pure oppor denno i migliori conduttori. Quando dunque un metallo tocca il disco elettrizato che ripofa sul suo piano, lo spoglia immantinente dell' elettricità fino al segno che la *tensione* diviene affatto *insensibile*, non però *nulla*, essendo ridotta supponiamo, a $\frac{1}{100}$ di grado. Ma se sollevando il disco in alto la sua *capacità* si ristringa a segno che dispieghi una tensione elettrica 100 e più volte maggiore, questa salirà dunque a 2 gradi, ed oltre; con che sarà divenuta sensibile, finanche al punto di dare una scintilla.

48. Fin qui considerato abbiamo come l'azione delle atmosfere elettriche debba modificare l'elettricità del disco nelle sue varie situazioni, allorchè gli è stata infusa prima di accostarlo al piano deferente. Ora vediamo che avvenir debba allorchè gli s'infonde stando già egli vicino o meglio applicato al detto piano. Quando ho detto dal bel principio (32.) che in tale stato egli ha molto

maggiore

maggiore *capacità*, e ſon venuto provandolo fin qui, ho detto e provato tutto: le applicazioni ſono facili a farſi. Gioverà non pertanto eſemplificare un eſperienza. Mi ſi dia una boccia di Leyden, o un ampio conduttore elettrizzati a 1 ſol grado di *tenſione*, od anche meno. Se io farò toccare l'una o l'altro al mio diſco poſato, è chiaro che gli comunicheranno della loro elettricità a miſura della ſua *capacità*, tanto cioè quant' egli puo riceverne per comporſi con eſſi ad una *tenſione* oſſia forza elettrica *eguale*, ſupponiamo di ½ grado. Ma la ſua *capacità* or ch' egli è non ſolamente *conjugato* ma combaciante il conduttore compagno, è 100 e più volte maggiore (46.) di quando ſi trova iſolato *ſolitariamente*, oſſia vi vuole per produrvi la data tenſione 100 volte maggior doſe di elettricità (33.), quindi appunto ne avrà preſo 100 volte più, che non avrebbe potuto prenderne ſtando iſolato in aria. Quando dunque ſi leverà in alto a miſura che allontanandoſi dal caro piano ſi ridurrà alla naturale ſua anguſta *capacità*, la *tenſione* elettrica diſpiegheraſſi maggiore, e maggior ſempre fino al termine di 50 gradi (nel ſuppoſto caſo che la tenſione foſſe di ½ grado ſtando il diſco poſato), quando cioè la ſua atmosfera non facendoſi più ſentire al detto piano, ſarà ceſſata ogni maniera di *compenſo*, e tolto quell' *equilibrio accidentale*, che teneva la tenſione così baſſa (39. 42.). E inutile il dire, che calando di nuovo il diſco verſo il piano, ſi abbatterà di nuovo l'elettrometro, a miſura che *l'equilibrio accidentale* ſi andrà riſtabilendo; giacchè queſto è il primo fenomeno che contemplato abbiamo (38.), e che ne ha condotti alla ſpiegazione di tutto il reſto.

49. Soggiugnerò queſto per ultimo ſchiarimento. Succede al diſco che paſſa dallo ſtato d'iſolamento ſolitario a quello di affacciarſi fin anche a combaciare un piano convenientemente preparato, o da queſto all' altro ſtato, lo ſteſſo che ſuccede ad un conduttore compreſo

compreso sotto angusta superficie, che si dispieghi in una assai più ampia, e *vice versa* (richiamiamo l'esempio della catena ammucchiata e poi distesa, o dei cilindri ch' entrano un nell' altro (35.). Elettrizzato a un alto grado il conduttore quand' è avvolto e impicciolito, se dopo viene a distendersi od allungarsi, decade in lui la *tensione* a misura che l'elettricità, compartendosi a una più grande *capacità*, vien diradata. All' incontro elettrizzato debolmente quando è disteso e gode della sua maggiore capacità, se dopo si avvolge e rappiccioliscе, va egli acquistando viemmagior *tensione* a misura che l'elettricità si raccoglie e viene condensata in una capacità minore. Così appunto il nostro disco se venga elettrizzato quand' è *solitario* a una forte *tensione* questa anderà scemando à misura ch' egli si affacia ad un altro piano non isolato; all' incontro elettrizzato debolissimamente quando è prossimo a questo piano o lo combacia, vedrassi crescere in lui insignemente la *tensione* a misura che si allontana da quel piano. Si puo dunque dire che l'elettricità viene qui pure in certo modo *condensata*, non altrimenti che nell' addotto esempio del conduttore che s'impiccioliscе: e quindi il nome di *condensatore* che ho dato al mio apparecchio. Certo se non puo dirsi nel nostro caso *condensata* l'elettricità in minore spazio, giacchè e massa e volume rimangono i medesimi nel disco che adoperiamo, ella è però confinata in tal corpo di cui la *capacità* di grandissima che era è divenuta come che sia piccolissima.

50. Ora se una debole insensibile forza elettrica di una boccetta di Leyden o di un conduttore appena un poco carichi applicata al disco giacente puo accumularvi tanto di elettricità, onde poi levato in alto dispieghi una forte tensione, vibri vivace scintilla, &c. che farà una carica forte della boccia o del conduttore applicatavi egualmente? Non farà gran cosa dippiù, per la ragione che tutta quell' elettricità ch' è superiore in forza alla

alla piccola resistenza che oppone la superficie del piano (46.), sia persa, trapassando in esso (47.). Ad ogni modo se questo piano essendo convenientemente preparato (11. 12. 22.), tale resistenza sia discreta, il disco non se ne staccherà senza vibrare d'attorno dagl' orli comunque ritondati fiocchi di luce, per la strabocchevole copia di elettricità, di cui si troverà carico: e a tanto non sarà neppur necessario che la boccetta che s'impiega a dargliela abbia assai forte carica, bastando una mediocre, e meno che mediocre, tale che appena giunga a dar scintilla.

51. Da tutto il fin qui detto s'intende facilmente, che se il disco posato puo prendere buona dose di elettricità da una boccia di Leyden *, o da un ampio conduttore, comechè debolissimamente animati, non lo puo in alcun modo da un conduttore poco capace (e come darebbe questi cio che non ha?) a meno che non si continui d'altra parte ad infondere a lui medesimo

* Nella mia memoria sulla capacità de' conduttori semplici dimostro la grandissima *capacità* che ha una boccia di Leyden comparativamente alla sua mole, appunto perchè l'elettricità che s'infonde ad una superficie truova un gran compenso nell' elettricità contraria che prende la superficie opposta, ciò che produce la solita diminuzione di *tensione*, &c. Vi fo vedere come 16 pollici quadrati di *superficie armata* hanno una capacità eguale a un conduttore di verghe inargentate lungo presso a 100 piedi, il quale ne ha una grandissima, talche le sue scintille producono la vera *commozione* in un grado abbastanza forte. Ivi anche accenno come tutti i fenomeni della carica e della scarica degli strati isolanti, dell' elettroforo, delle punte ec. possono dipendere dall' istessa azione delle atmosfere elettriche, combinata, per ciò che appartiene agli strati isolanti, con una certa non molto grande resistenza che prova l'elettricità ad affiggersi alla superficie di questi egualmente che a sortirne, e con quella incomparabilmente più grande e puo dirsi insuperabile che la impedisce di diffondersi attraversandone la spessezza. Intorno a che fin dal tempo in cui pubblicai la descrizione, e le principali sperienze del mio elettroforo, che fu nel 1775 (vegg. la Scelta d' Opusc. interess. di quell' anno) io avea promesso di esporre tutte le mie idee in un trattato che avrebbe per titolo: *dell' azione delle atmosfere elettriche, e de' fenomeni che ne derivano negli strati isolanti.*

quella

quella qualunque debole elettricità, a meno che la sorgente non continui per qualche tempo; il che ha luogo per esempio nel conduttore atmosferico che bee l'elettricità insensibile dell' aria, e in quello malissimo isolato d'una macchina ordinaria, il di cui giuoco vi mantiene una sì debole *tensione* di elettricità, che in niun modo appara. In ambi questi casi abbiamo osservato infatti (4. 25.) che vi vuol del tempo prima che il disco possa raccorre una dose sufficiente di elettricità.

52. Come un ampio conduttore trasmette la massima parte della sua elettricità al nostro disco, il quale quantunque assai più picciolo, gode però in grazia della sua vantaggiosa posizione, in grazia di quell' *equilibrio accidentale* a cui si compone col piano, d'una *capacità* molto più grande di quella che gli compete in istato solitario; e come levando in seguito esso disco in alto, con che tolto ogni *equilibrio o compenso*, vien ristretto alla naturale sua angusta capacità, quella stessa dose di elettricità presa al gran conduttore, e che appunto per esser egli sì grande vi producea sì debole *tensione*, or ne produce una tanta più grande in cotesto disco; nell' istessa maniera, e per l'egual ragione l'elettricità aumenterà una seconda volta di tensione facendola passare dal disco già sollevato ad un altro giacente molto più piccolo, da innalzarsi quindi similmente.

Il Sig. CAVALLO, a cui dietro le altre mie sperienze, suggerì quest' artificio, ha fatto tal picciolo disco d'una laminetta non più grande d'uno scillino. E certo questo secondo *condensatore* dell' elettricità è utile in molti casi in cui l'elettricità non è sensibile ancora o dubbia col primo: come ce ne hanno assicurato varie prove che facemmo insieme. Talora l'ordinario disco toccato dal corpo, di cui si dubitava se avesse o no un principio di elettricità, non movea ancora l'elettrometro sensibilissimo dell' istesso Sig. CAVALLO; ma toccato con quel disco l'altro picciolino, questo facea divergere sensibilmente le

palottoline dell' elettrometro. Eppure qualche volta anche con queſto non ſi otteneva nulla, o un' ombra ſolamente di elettricità. Or ſe noi ſupponiamo la *tenſione* elettrica accreſciuta a 1000 volte tanto per l'intervento dei due condenſatori, il che non è troppo, quanto mai debole eſſer dovea originariamente nel corpo eſaminato? Quanto debole p. e. quella che ſi eccita in un metallo ſtrofinandolo colla mano nuda, giacchè communicata al primo, e da queſto al ſecondo picciolo diſco, e finalmente all' elettrometro, le palle appena fan cenno di ſcoſtarſi? Ma baſta che facciano tanto per eſſer noi convinti, che l'elettricità non è nulla, e che il metallo l'ha originariamente contratta per lo ſtroppicciamento della mano. Quanto mai eravam lontani da una ſimile ſcoperta pochi anni addietro prima del noſtro *condenſatore*, e dell' elettrometro così ſenſibile del Sig. CAVALLO. Quanti gradi di elettricità noi ſcopriamo adeſſo al diſotto del più picciolo d'allora?

APPENDICE.

HO detto al § 28. che mi è riuſcito finalmente di ottenere ſegni diſtintiſſimi di elettricità e dalla ſemplice evaporazione dell' acqua, e da varie efferveſcenze chimiche. Eſſendo queſto un fatto non meno intereſſante che nuovo, ſtimo non inopportuno di far qui il racconto fedele delle ſperienze. Le prime dunque, come ivi accenno, ſono ſtate fatte a Parigi in compagnia di due fiſici illuminati e membri dell' Acc. R. delle Scienze. Furono queſti il Sig. LAVOISIER, e il Sig. DE LA PLACE. Eglino concepiron meco la ſperenza di un felice riuſcimento

quando

quando ebbi loro mostrato gli effetti del mio *condensatore*, e spiegata la ragione dei fenomeni: conseguentemente il Sig. LAVOISIER ne ordinò un grande col piano di marmo bianco. I primi tentativi da me fatti con questo in compagnia del Sig. DE LA PLACE sull' evaporazione dell' acqua e dell etere non furono coronati dal successo; ma il tempo era cattivo, la stanza troppo picciola e ingombrata di vapori, e l'apparato non troppo ben in ordine. All incontro quelli che ripeterono l'istesso Sig. DE LA PLACE e Sig. LAVOISIER ad una campagna di quest' ultimo ebbero buon riuscimento. La qual cosa c'invogliò a ripetere e moltiplicar le sperienze, e il successo fu completo, avendo ottenuto segni chiarissimi di elettricità dall' evaporazione dell' acqua, dalla semplice combustione dei carboni, e dall' effervescenza delle limature di ferro nell' acido vitriolico diluto. Ciò avvenne il giorno 13 Aprile e la maniera di far l'esperienza fu questa: si isolò in un aperto giardino una gran lastra di metallo, alla quale era attaccato un lungo filo di ferro che veniva a terminare in contatto dello scudo o disco posato sul piano di marmo, e questo tenevasi continuamente asciutto e caldo da alquanti carboni sottoposti. Ciò fatto posimo su la detta lastra isolata alcuni scaldini ripieni di carboni mezzo accesi, e lasciammo che la combustione ajutata da un gentil vento che spirava andasse rinforzandosi per alcuni minuti: allora rimovendo lo scudo dal contatto del filo metallico e quindi da quello dal marmo con alzarlo al consueto modo, vi comparvero i segni aspettati di elettricità, mentre accostato al nuovo elettrometro del Sig. CAVALLO, fece che s'aprissero i due fili colle pallottoline: esaminata questa elettricità si trovò essere *negativa*. Si ripetè l'esperienza ponendo sulla lastra isolata invece dei scaldini quattro vasi con entro limatura di ferro e acqua, quindi versando in tutti quattro a un tempo abbastanza d'olio di vitriolo per far sorgere

una furiosa effervescenza: quando il più forte bollore cominciava a cadere, allora fu che esplorato lo scudo non che movere i fili dell' elettrometro a qualche distanza, ci diede una sensibile scintilla. Anche qui l'elettricità fi riconobbe essere *negativa*. Quanto furon vivi e distinti i segni elettrici con tal prova dell' effervescenza, altrettanto deboli ed equivoci riuscirono questa volta coll' evaporazione dell' acqua eccitata or con mettere delle casserole con entro acqua a bollire sopra i scaldini portati come qui innanzi dalla lastra isolata, ora con versar l'acqua in coteste casserole previamente ben riscaldate.

Pochi giorni dopo ripetemmo le sperienze in una grande stanza estendendole alle altre effervescenze che producono l'aria fissa, e l'aria nitrosa, con buon successo: l'evaporazione sola dell' acqua produsse segni debolissimi talchè ebbimo pena a determinare di quale specie fosse l'elettricità; anzi di tre volte, due ci parve che fosse *positiva*; ma v' è luogo a credere, ed io giudico certamente, che sia stato un errore.

Ancor passati alcuni giorni si ritornò alle sperienze essendo di compagnia anche il Sig. LE ROY membro esso pure dell' Accademia R.; ma nè la combustione, nè l'evaporazione dell' acqua non ci dieder segni sensibili: di che accagionammo l'esser l'aria umidissima per il tempo piovoso che faceva. Pur ne ottenemmo colla generazione dell' aria infiammabile nel momento della più viva effervescenza: e se l'elettricità non fu questa volta così forte da scintillare, lo fu abbastanza perchè ne distinguessimo chiarissimamente la specie, che era *negativa*.

Prima di lasciar Parigi (che fu il 23 Aprile) volendo io mostrare qualche sperienza di questo genere ad un amatore di elettricità e valente machinista, il Sig. BILLAUM, una volta che mi trovai nel suo laboratorio, presi una giara di vetro, e sospesala a un cordoncino di seta vi misi i materiali per la produ-

zione dell' aria infiammabile: avea fatto entrare nella giara medesima un filo di ferro in modo che toccasse la limatura e l' altro suo capo sporgente venisse a communicare coll' elettrometro sensibilissimo del Sig. CAVALLO. Quando l'effervescenza fu salita al sommo e la spuma formontava i labbri del vaso, le palle, scostandosi, dieder segno di elettricità; nè questa fu così debole, che non potesse conoscersi esser *negativa*.

Le sperienze coll' evaporazione dell' acqua, che non avean troppo bene corrisposto a Parigi, ebbero molto miglior successo a Londra, quando mi suggerì l'espediente di gettere dell' acqua sopra i carboni accesi ch' erano in uno scaldino isolato. L'effumazione rapida che succede non manca mai di elettrizzare lo scaldino *negativamente*, il quale dà segni abbastanza sensibili col solo elettrometro, e col condensatore, se è ben preparato, arriva a produr scintille. Si trovarono presenti la prima volta a queste sperienze in casa del Sig. BENNET grand' amatore di elettricità, il Sig. CAVALLO e il Sig. KIRWAN membri della S. R. e il Sig. WALKER lettore di fisica. Ci servimmo per apparecchio condensatore d'un picciolo scudo d'elettroforo, e d'un piattello di legno, che si trovò al giusto punto semicoibente, il che è raro quando il legno non è inverniciato.

Un' altra volta in casa del Sig. CAVALLO riuscì l'esperienza isolando un picciolo crogiuolo con entro due o tre carboni accesi e quindi versandovi un cucchiazo d'acqua: un filo di ferro che toccava i carboni, ed estendevasi fino all' elettrometro, vi portò sensibile elettricità e sempre *negativa*.

Queste sono le sperienze, che fino ad ora ho avuto occasione di fare; intorno alle quali non debbo tralasciar di dire, che sebbene non avessimo sempre bisogno dell' apparecchio *condensatore* (il quale, se non è benissimo in ordine, a nulla serve, e può nuocere anzichè giovere) per aver segni non dubbi, il solo elet-

elettrometro sensibilissimo del Sig. CAVALLO avendoci bastato più volte; convien però confessare che si fu quell' apparecchio che ci mise sulla via di tali sperienze, e che col mezzo suo solamente potemmo ottenere segni di una certa forza, e fin la scintilla elettrica. Io non dubito che essendo ora rese così facili tali sperienze, non siano per essere e ripetute e promosse. Il campo è solamente aperto, e molto resta ancora a fare. Se i corpi risolvendosi in vapori o in un fluido elastico si caricano di fuoco elettrico a spese degl' altri corpi, e gli elettrizzano per conseguenza *negativamente*, venendo in seguito i vapori medesimi a condensarsi, non cercheranno essi di deporre questo carico, e non produrranno conseguentemente segni di elettricità *positiva?* Ecco ciò che merita singolarmente d'essere verificato coll' esperienza. Io ho già immaginato diversi modi di tentare la cosa che metterò alla prova tosto che ne abbia il commodo. Intanto mi sia qui permesso di dar corso per un momento alle idee che volgo in mente intorno all' elettricità atmosferica.

Le sperienze fatte fin quì, e che abbiamo riferite, benchè non sian molte, tutte però concorrono a mostrarci che i vapori dell' acqua, e generalmente le parti d'ogni corpo, che si staccano volatilizzandosi, portano via seco una quantità di fluido elettrico a spese dei corpi fissi che rimangono, elettrizzandoli con ciò *negativamente*, non altrimenti che ne portan via una quantità di fuoco elementare, con ciò raffreddandoli. Quindi volli inferire che i corpi risolvendosi in vapori, o prendendo l'abito aereo, acquistino una maggiore capacità rispetto al fluido elettrico, giusto come l'acquistano maggiore rispetto al fuoco comune o fluido calorifico. Chi non sarà colpito da così bella analogia, per cui l'elettricità porta del lume alla novella dottrina del calore, e ne riceve a vicenda? Parlo della dottrina dal calor *latente* o *specifico*, come si vuol chiamare, di cui BLACK e

WILKE

WILKE colle stupende loro scoperte han gettato i semi, e che è stata ultimamente tanto promossa dal Dr. CRAWFORD dietro le sperienze del Dr. IRWINE.

Seguendo questa analogia siccome i vapori allorchè si condensano e ritornano in acqua, e conseguentemente alla primiera più angusta capacità, perdono il lor calore *latente*, ossia depongono il dippiù di fuoco che si avevano appropriato volatizzandosi; così pure daran fuori il fluido elettrico divenuto ora ridondante. Ed ecco come nasce l'elettricità di eccesso, che domina sempre più o meno nell' aria anche serena, a quell' altezza che i vapori cominciano a condensarsi; la quale è più sensibile nelle nebbie, ove quelli si condensano maggiormente, e già si figurano in goccie; e infino fortissima laddove le folte nebbie si agglomerano in nubi. Fin quì l'elettricità dell' atmosfera sarà sempre *positiva*. Ma formata che sia una nube potentemente elettrica in più, ella avrà una sfera di attività intorno ad essa, nella quale se avviene ch' entri un' altra nube, allora giusta le note leggi delle atmosfere, gran parte del fluido elettrico di questa seconda nube si ritirerà verso l'estremità più lontana dalla prima, e potrà anche sortirne ove incontri o altra nube, o vapori, o prominenze terrestri che lo possan ricevere: ed ecco una nube elettrizzata *negativamente*, la quale potrà a sua posta occasionare coll' influsso della propria atmosfera l'elettricità *positiva* in una terza, &c. di questa maniera s'intende benissimo come si possano avere sovente nè conduttori atmosferici segni di elettricità *negativa* a cielo più che coperto; e come ne' temporali specialmente, ove molte nubi si veggono pensili e staccate vergere al basso, e or ondeggiare per qualche tempo, ora scorrere le une sotto le altre, or trasportarsi rapidamente, l'elettricità cambj più volte, e spesso a un tratto da *positiva* in *negativa*, e vice-versa.

Or

Or anche non fia più ſtupore che le eruzioni de vulcani, ſiano ſtate ſovente accompagnate da fulmini in iſpecie. Quella ſtrepitoſiſſima del Veſuvio dell' anno 1779, in cui infinite ſaette ſi ſon vedute guizzare entro gl' immenſi globi di fumo eruttati. Le poche ſperienze fatte mi han dato a vedere che la quantità di elettricità prodotta dalle effumazioni, dipenda molto e dalla copia dei fumi che s'alzano e ſingolarmente dalla rapidità. Or quale e quanta non dee eſſere l'elettricità in ſimili eruzioni?

XVII. *Extract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1780. *By* Thomas Barker, *Eſquire.*

Read April 11, 1782.

		Barometer.			Thermometer.						Rain.
					In the Houſe.			Abroad.			
		Higheſt.	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	30,15	28,55	29,45	46½	32	38	47	21	32	2,264
	Aftern.				46	33	38	50½	25	36	
Feb.	Morn.	29,82	28,32	29,29	46½	38	42½	45	30	37½	1,664
	Aftern				47	39	43	52½	36½	44	
Mar.	Morn.	30,01	29,46	29,74	51	40	46	44½	30	38	0,160
	Aftern.				53½	42	47½	56½	40	48	
Apr.	Morn.	29,88	28,92	29,43	57	41	50	53	33½	43	1,938
	Aftern.				58	43	51½	64	39	54	
May	Morn.	29,95	29,20	29,60	66	47	54	64	38	47	0,974
	Aftern.				70	48	55	76½	47½	59½	
June	Morn.	29,88	29,00	29,40	69	58	62	69	52	60	2,958
	Aftern.				73	59	63½	79½	57	67	
July	Morn.	29,96	29,22	29,59	68	61	64	67	49	58½	1,683
	Aftern.				71½	62	66	79½	62	70½	
Aug	Morn.	29,89	29,02	29,45	70½	60	65	64	49½	57	1,097
	Aftern.				72½	61	66½	80	61	71	
Sept.	Morn.	29,86	29,04	29,45	65	50½	59	63	38	52	4,004
	Aftern.				67	51	60	80	50½	63	
Oct.	Morn.	30,00	28,96	29,67	59	44	52½	59	26½	44	0,081
	Aftern.				61	44	53	66	41	53½	
Nov.	Morn.	29,74	28,49	29,22	51	40½	45	52	28	39	2,296
	Aftern.				51½	41	46	54	35	45	
Dec.	Morn.	29,68	29,00	29,34	50	37½	43½	51	28½	39	1,703
	Aftern.				51	37	44	53	33½	42½	
											20,822

The ground was unuſually dry the firſt half of January, and want of water in ſome places. Inclined to froſt, but not fixed; one for a week was followed toward the end of the month by a ſmarter but ſhorter with a good deal of ſnow, and went away with rain and floods; then ſhowery, and in February often ſtormy. The month of March was drier, generally fine, a good ſeed-time, and the middle of the month warm and growing; but cold N.E. winds at the end. Soon after April came in, the weather was fine and growing, ſometimes ſhowery, and the middle being very warm brought on things very much; but the end of the month, and beginning of May, cold N.E. winds, yet ſometimes hot ſun. About the middle of May was ſhowery and growing; but the end of it, and beginning of June, hot, dry, clear, and burning June 3d, began ſhowers and often thunder, which being ſeveral times repeated for a fortnight, and hot weather with it, made plenty of graſs in this country and ſome others; but where the rains were ſmaller, as they were about London, in Suſſex, and other places, it only freſhened the ground without growing much. The beginning of hay-time was very fine, a wet week in the middle did not greatly hurt the hay, and was very good for the turnips, then newly ſown. The latter half of July, and almoſt all Auguſt, were very dry, hot, and burning; the harveſt almoſt all very well got, except ſome of the peaſe, which were either not got in, or the hovels not thatched, when the great rain came September 2. There was a great crop of wheat, but in ſome places it was mildewed; the barley was univerſally good; the peaſe uncertain. The general prices were, wheat thirty-eight ſhillings, barley ſixteen or ſeventeen ſhillings, oats eleven or twelve ſhillings, peaſe and beans about twenty-two or twenty-three ſhillings a quarter.

The ground was now exceedingly burnt, and great want of water; but in the firſt half of September there came ſo much rain, the ground already warm, and hot weather with it, that in a fortnight's time there was plenty of graſs. The autumn was in general fine and pleaſant, and the latter end of September and all October ſcarce any rain, ſo that at the end of the month the graſs in ſome places plainly began to burn, which was remarkable ſo late in the year, and after its being ſo fruitful in September; and notwithſtanding ſome ſhowery weather in the former part of November, the roads were rather duſty the beginning of December; then dark, miſty, raw, cold eaſt winds, but milder again and ſhowery till the end of the year. It has been an uncommonly open ſeaſon, there have been ſome froſty mornings, and one day or two it ſcarce went away in the ſhade; but there has not been one thorough froſty day yet, and there has been thunder at Chriſtmas.

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THE ROYAL SOCIETY

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1781.

	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Jan. 1	8	0	45,0	47,0	29,80		SW	1	Cloudy.
	2	0	46,0	47,0	29,71		SW	1	Cloudy and rain.
2	8	0	34,5	43,5	29,57	0,045	W by N	1	Fair.
	2	0	40,0	45,0	29,61		NW	1	Fine.
3	8	0	33,5	38,0	29,76		N by W	1	Fair.
	2	0	38,0	39,0	29,88		N by W	1	Fine.
4	8	0	28,0	35,0	30,01		NW	1	Fair and frosty.
	2	0	34,0	36,0	30,05		NW	1	Cloudy.
5	8	0	25,0	32,0	30,20		SSW	1	Frosty.
	2	0	32,0	33,0	30,23		SW	1	Frosty.
6	8	0	35,0	33,5	30,21		SSW	1	Cloudy.
	2	0	40,0	36,5	30,25		SW	1	Cloudy.
7	8	0	38,5	37,0	30,22		SSW	1	Cloudy.
	2	0	46,0	40,5	30,10		W by S	1	Fair.
8	8	0	40,5	41,5	30,31	0,042	N by E	1	Fair.
	2	0	42,5	41,5	30,39		ENE	2	Cloudy.
9	8	0	32,5	37,5	30,54		NNE	2	Fair.
	2	0	37,5	38,5	30,55		NE	2	Fine.
10	8	0	32,0	34,0	30,52		E by N	2	Cloudy.
	2	0	33,5	35,0	30,50		E by N	2	Cloudy.
11	8	0	32,0	33,5	30,38		E by N	3	Cloudy.
	2	0	31,0	33,5	30,32		NE	3	Cloudy.
12	8	0	31,5	32,0	30,26		ESE	1	Foggy.
	2	0	35,0	35,0	30,24		SE	1	Cloudy.
13	8	0	33,0	34,0	30,11		ENE	1	Foggy.
	2	0	35,0	35,0	30,16		NE	1	Cloudy.
14	8	0	29,0	32,0	30,04		E by N	1	Foggy.
	2	0	34,0	35,0	29,98		ENE	1	Cloudy.
15	8	0	30,5	33,5	29,92		ENE	1	Foggy.
	2	0	36,0	35,0	29,89		NE	1	Fine.
16	8	0	31,0	33,5	29,89		ENE	1	Foggy.
	2	0	33,5	33,5	29,83		NE	1	Cloudy.

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METEOROLOGICAL JOURNAL

for January 1781.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	32,5	33,0	29,82		ENE	1	Foggy.
	2	0	38,0	35,0	29,76		NE	1	Rainy.
18	8	0	40,0	37,0	29,57	0,190	E by N	1	Rainy.
	2	0	45,0	39,0	29,56		NE	1	Cloudy.
19	8	0	37,5	40,0	29,78	0,221	NNE	1	Fine.
	2	0	41,0	41,5	29,86		NNE	1	Fine.
20	8	0	34,0	38,0	30,05		SE	1	Foggy.
	2	0	41,0	40,0	29,99		SE	1	Fine.
21	8	0	44,0	40,0	29,45		SW	1	Foggy.
	2	0	41,5	42,5	29,44		NNE	1	Rainy.
22	8	0	32,0	38,0	29,65	0,073	SE	1	Fair.
	2	0	33,0	38,0	29,66		SE	1	Fine.
23	8	0	26,0	32,0	29,39		ENE	1	Foggy.
	2	0	32,0	34,0	29,37		SE	1	Cloudy with snow.
24	8	0	35,5	34,0	29,16		SW	1	Rainy.
	2	0	41,0	37,0	29,04		SW	1	Rainy.
25	8	0	31,5	33,0	29,32		NW	1	Fair.
	2	0	35,0	36,0	29,52		NW	1	Fine.
26	8	0	35,0	34,0	29,09	0,485	SSE	1	Rainy.
	2	0	37,0	38,0	29,23		SE	1	Cloudy.
27	8	0	26,5	32,5	30,05		WSW	1	Fine.
	2	0	36,0	37,0	30,13		SW	1	Fine.
28	8	0	46,5	37,0	29,93	0,063	SW	2	Cloudy.
	2	0	51,5	42,0	29,94		SSW	2	Cloudy.
29	8	0	46,5	43,0	29,89		SSW	2	Cloudy.
	2	0	50,5	46,0	29,89		SW	2	Cloudy.
30	8	0	47,0	47,5	29,75	0,229	SW	2	Cloudy.
	2	0	48,0	49,0	29,81		SW	3	Showery.
31	8	0	35,5	43,5	30,22		WNW	1	Fine.
	2	0	45,0	44,0	30,22		SW	1	Fine.

METEOROLOGICAL JOURNAL

for February 1781.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inche.	Inch.	Points.	Str.	
Feb. 1	8 0	42,0	43 0	30,05	0,110	SSW	1	Rainy.
	2 0	49,0	46,0	30.01		NW	1	Fine.
2	8 0	40,5	43,0	30 19	0,120	SW	1	Foggy.
	2 0	46,0	46,0	30,12		NW	1	Rainy.
3	8 0	41,5	44,0	30,33		SSW	1	Cloudy.
	2 0	43,0	46,0	30,34		SW	1	Cloudy.
4	8 0	43,0	46,0	30,34		SW	1	Cloudy.
	2 0	48,0	47,0	30,05		SW	1	Fine.
5	8 0	44,5	44,0	29,78		SW	1	Fine.
	2 0	50,0	49,0	29,75		SW	1	Cloudy.
6	8 0	46,5	49,0	29 88		SW	1	Cloudy.
	2 0	52,0	51,0	29,96		SW	1	Fine.
7	8 0	48,5	46,5	30,03		WSW	1	Fine.
	2 0	40,0	43,0	29,73		SW	2	Cloudy.
8	8 0	47,5	48,5	29,51	0,233	SSE	2	Rainy.
	2 0	52,0	50,5	29.59		SW	2	Fine.
9	8 0	40,0	46,0	29,82		SW	1	Fine.
	2 0	52,5	50,0	29 83		SW	1	Cloudy.
10	8 0	48,0	49,0	29,88		SSW	3	Cloudy.
	2 0	50,0	50,0	29,74		SW	3	Cloudy.
11	8 0	42,5	47,0	29,58	0,081	SW	2	Fine.
	2 0	51,0	50,5	29 59		SW	2	Fine.
12	8 0	47,0	49,0	28,95	0,430	SW	3	Cloudy.
	2 0	44,0	46,0	29,96		SW	1	Cloudy.
13	8 0	43,0	45 0	29,36		SW	2	Cloudy.
	2 0	47,0	47,5	29,39		NW	2	Fine.
14	8 0	39,5	45,0	29 59	0,150	WSW	1	Cloudy.
	2 0	44,5	44,0	29.52		NW	2	Fine.
15	8 0	35,0	41,0	29,98		NW	1	Fine.
	2 0	43 5	44,0	30,05		SW	1	Fine.
16	8 0	33,0	37,0	30,09		WSW	1	Fine.
	2 0	44,0	42,0	30,02		NW	1	Fine.

METEOROLOGICAL JOURNAL

for February 1781.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8 0	31,5	37,5	30,12		NW	1	Fine.
	2 0	42,0	44,0	30,07		NW	2	Fine.
18	8 0	31,0	36,0	30,09		N by W	1	Fair.
	2 0	43,5	40,5	30,03		SW	1	Cloudy.
19	8 0	37,0	39,0	29,79		NW	1	Fine.
	2 0	43,5	42,5	29,81		NW	1	Cloudy.
20	8 0	37,0	39,0	30,22		SW	1	Cloudy.
	2 0	42,5	41,5	30,23		NE	1	Fine.
21	8 0	34,0	38,0	30,16		NNE	1	Cloudy.
	2 0	35,5	39,5	30,09		NE	1	Rainy.
22	8 0	33,0	36,0	30,12	0,073	NNE	1	Fine.
	2 0	41,5	39,0	30,14		NNE	1	Fine.
23	8 0	37,5	37,0	30,03		NW	1	Cloudy.
	2 0	43,5	41,0	29,89		NW	1	Cloudy.
24	8 0	40,5	42,0	29,71	0,131	SW	1	Cloudy.
	2 0	49,0	44,0	29,71		SW	1	Rainy.
25	8 0	43,0	45,0	29,17	0,150	NW	1	Fine.
	2 0	44,0	46,5	29,19		NW	1	Fine.
26	8 0	37,0	41,0	29,08		SW	1	Cloudy.
	2 0	41,5	43,0	29,08		SW	1	Fine.
27	8 0	39,0	41,0	29,26	0,072	SW	1	Cloudy.
	2 0	45,0	47,0	29,88		NW	3	Cloudy.
28	8 0	37,5	41,0	29,79	0,126	NW	1	Fine.
	2 0	45,0	46,0	29,97		NW	1	Fine.

METEOROLOGICAL JOURNAL
for March 1781.

	Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Mar. 1	7	0	35,0	41,5	30,19		SW	1	Fine.
	2	0	47,0	43,5	30,06		SW	1	Rainy.
2	7	0	41,0	43,0	30,26	0,120	WSW	1	Cloudy.
	2	0	50,5	47,0	30,26		NW	1	Fine.
3	7	0	48,0	49,0	30,26		SW	1	Cloudy.
	2	0	55,0	51,0	30,26		WSW	1	Cloudy.
4	7	0	47,0	51,0	30,18		SW	1	Cloudy.
	2	0	50,5	51,5	30,10		SW	1	Cloudy.
5	7	0	45,5	51,5	30,14		SW	1	Cloudy.
	2	0	51,0	51,5	30,22		NE	1	Cloudy.
6	7	0	37,0	47,0	30,36	0,002	SW	1	Fine.
	2	0	52,5	51,0	30,33		SW	1	Fine.
7	7	0	35,5	45,5	30,25		SW	1	Cloudy.
	2	0	51,0	49,0	30,19		WSW	1	Fine.
8	7	0	37,5	40,0	30,11		SSW	1	Rainy.
	2	0	48,0	47,5	30,19		SSW	1	Cloudy.
9	7	0	43,0	48,5	30,22	0,170	SW	1	Fine.
	2	0	55,0	50,5	30,17		SSW	1	Fine.
10	7	0	44,5	49,5	30,17		WSW	1	Fine.
	2	0	55,0	52,0	30,17		NW	1	Cloudy.
11	7	0	38,5	48,5	30,19		NNE	1	Fine.
	2	0	51,5	50,0	30,21		NNE	1	Fine.
12	7	0	32,0	42,0	30,27		SE	1	Cloudy.
	2	0	42,0	46,0	30,28		NW	1	Cloudy.
13	7	0	41,0	44,5	30,32		NE	1	Fine.
	2	0	49,5	48,0	30,31		SE	1	Fine.
14	7	0	32,5	38,5	30,33		NE	1	Cloudy.
	2	0	45,5	44,0	30,33		ESE	1	Fine.
15	7	0	33,5	37,0	30,44		NE	1	Fine.
	2	0	48,5	47,0	30,46		ENE	1	Fine.
16	7	0	36,0	37,5	30,39		ENE	1	Cloudy.
	2	0	46,5	46,0	30,34		ENE	1	Fine.

METEOROLOGICAL JOURNAL

for March 1781.

	Time.	Therm without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Mar. 17	7 0	39,0	38,5	30,22		ESE	1	Cloudy.
	2 0	50,5	48,0	30,16		ENE	1	Fine.
18	7 0	33,0	41,5	30,09		SSE	1	Foggy.
	2 0	57,0	47,0	30,06		WSW	1	Fine.
19	7 0	39,0	46,5	30,14		NW	1	Fine.
	2 0	55,0	49,0	30,14		WSW	1	Fine.
20	7 0	41,5	44,5	30,22		NNW	1	Foggy.
	2 0	60,0	51,0	30,22		NW	1	Fine.
21	7 0	45,0	50,0	30,18		W by S	1	Fine.
	2 0	57,5	5,,0	30,16		WSW	1	Fine.
22	7 0	41,5	46,5	30,19		W SW	1	Fine.
	2 0	58,0	53,5	30,18		SW	1	Fine.
23	7 0	41,5	46,5	30,38		SE	1	Fine.
	2 0	51,0	50,5	30,35		ENE	1	Fine.
24	7 0	36,0	42,0	30,47		NW	1	Foggy.
	2 0	56,0	49,0	30,47		SE	1	Fine.
25	7 0	38,0	46,0	30,46		NE	1	Fine.
	2 0	60,5	51,0	30,29		SW	1	Fine.
26	7 0	39,0	43,0	29,91		WSW	1	Fine.
	2 0	47,0	50,5	30,01		NE	2	Rainy.
27	7 0	48,5	43,5	30,02		NW	1	Fine.
	2 0	49,0	44,0	29,93		NW	2	Fine.
28	7 0	34,0	39,0	29,88		NE	1	Fine.
	2 0	48,0	45,0	29,89		NE	2	Fine.
29	7 0	34,0	37,0	29,92		NE	2	Cloudy.
	2 0	43,0	42,0	29,91		NE	2	Fine.
30	7 0	35,5	37,5	29,91		NE	2	Fine.
	2 0	47,0	42,0	29,91		NE	2	Fine.
31	7 0	35,5	35,0	30,09		ENE	1	Foggy.
	2 0	34,0	43,5	30,09		NE	2	Cloudy.

METEOROLOGICAL JOURNAL
for April 1781.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 1	7	0	37,5	38,0	29,91		ESE	1	Fine.
	2	0	37,9	43,0	29,81		E by S	1	Fine.
2	7	0	37,9	42,0	29,74		SW	1	Fine.
	2	0	53,5	47,0	29,83		ENE	1	Fine.
3	7	0	37,5	44,5	29,98		NE	1	Fine.
	2	0	53,5	52,0	29,94		SE	1	Fine.
4	7	0	38,0	43,0	29,76		NE	1	Cloudy.
	2	0	43,5	44,0	29,63		NE	1	Cloudy.
5	7	0	40,9	43,5	29,34		SE	1	Cloudy.
	2	0	51,0	48,0	29,29		SE	1	Cloudy.
6	7	0	45,9	48,5	29,37		SW	1	Cloudy.
	2	0	50,5	49,5	29,38		SW	1	Fine.
7	7	0	46,9	49,0	29,57		SW	1	Fine.
	2	0	51,9	51,5	29,51		SE	1	Rainy.
8	7	0	48,9	50,5	29,53	0,150	SW	2	Cloudy.
	2	0	50,9	65,0	29,68		SW	2	Fine.
9	7	0	53,9	54,0	29,72	0,200	SW	2	Rainy.
	2	0	50,9	60,0	29,82		NW	2	Fine.
10	7	0	54,0	57,5	29,74		SW	1	Rainy.
	2	0	62,5	60,0	29,76		SW	2	Fine.
11	7	0	49,0	51,5	29,73	0,140	SE	1	Fine.
	2	0	57,0	56,0	29,66		SE	1	Cloudy.
12	7	0	49,0	54,5	29,52		NW	1	Cloudy.
	2	0	61,5	58,0	29,64		NW	2	Cloudy.
13	7	0	39,5	51,0	29,96	0,095	SSW	1	Fine.
	2	0	54,0	53,5	29,99		SW	1	Fine.
14	7	0	40,0	45,0	29,91		WSW	1	Fine.
	2	0	58,0	52,5	29,79		SW	1	Fine.
15	7	0	46,0	47,0	29,79		SW	1	Fine.
	2	0	62,0	66,5	29,81		SW	1	Fine.
16	7	0	46,0	49,0	29,88		SW	1	Fine.
	2	0	53,0	58,5	29,86		SSW	1	Fine.

METEOROLOGICAL JOURNAL

for April 1781.

	Time.		Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	49,0	53,5	29,91		SW	1	Fine.
	2	0	66,5	61,0	29,92		SSE	1	Fine.
18	7	0	48,0	53,0	29,94		ENE	1	Fine.
	2	0	65,0	62,5	29,91		ESE	1	Fine.
19	7	0	55,5	57,5	29,93		SSW	1	Fine.
	2	0	65,0	62,5	29,99		WSW	1	Fine.
20	7	0	51,0	67,0	30,02	0,065	SSE	1	Cloudy.
	2	0	64,0	64,0	30,09		W	1	Fine.
21	7	0	55,0	58,0	30,18		SSW	1	Rainy.
	2	0	64,0	63,0	30,16		SW	1	Cloudy.
22	7	0	55,5	58,0	30,09		SSW	1	Fine.
	2	0	66,0	64,0	30,05		SW	1	Fine.
23	7	0	48,0	52,0	30,07		SW	1	Fine.
	2	0	57,0	57,0	30,01		NW	1	Cloudy.
24	7	0	45,0	47,0	30,16		NNE	1	Cloudy.
	2	0	51,0	55,0	30,22		NE	2	Cloudy.
25	7	0	44,5	50,0	30,24		NW	1	Fine.
	2	0	46,5	49,0	30,34		NW	1	Fine.
26	7	0	43,0	47,0	30,32		NW	1	Fine.
	2	0	53,0	53,0	30,32		NW	2	Cloudy.
27	7	0	47,5	48,5	30,28		NNE	1	Fine.
	2	0	51,0	50,5	30,22		N by W	1	Fine.
28	7	0	42,0	42,0	30,09		N by E	1	Fine.
	2	0	57,0	54,5	29,96		N by E	1	Fine.
29	7	0	47,0	45,5	29,91		NNE	1	Fine.
	2	0	59,0	55,5	29,89		NNE	1	Fine.
30	7	0	44,0	46,5	29,92		NE	1	Cloudy.
	2	0	61,5	57,0	29,87		NNE	1	Cloudy.

METE-

METEOROLOGICAL JOURNAL

for May 1781.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 1	7	0	48,0	52,5	29,96		NNW	1	Cloudy.
	2	0	61,0	68,5	29,96		NE	1	Fine.
2	7	0	46,0	49,5	30,05		SE	1	Fine.
	2	0	62,0	59,0	30,05		SE	1	Fine.
3	7	0	48,5	49,0	30,01		ESE	1	Rainy.
	2	0	54,5	55,0	29,98		NE	1	Cloudy.
4	7	0	46,5	50,0	29,93	0,059	NE	1	Rainy.
	2	0	53,0	50,0	29,93		NE	1	Cloudy.
5	7	0	43,5	46,5	30,04	0,040	NE	1	Fine.
	2	0	52,0	51,5	30,06		ESE	1	Fine.
6	7	0	43,0	44,0	30,17		ENE	1	Cloudy.
	2	0	51,0	50,0	30,16		ENE	1	Fine.
7	7	0	43,5	44,0	30,14		SE	1	Fine.
	2	0	52,0	49,0	30,12		NE	1	Cloudy.
8	7	0	42,0	44,0	29,97		ESE	1	Fine.
	2	0	51,0	49,5	29,88		NNW	1	Fine.
9	7	0	43,0	47,0	29,72		NE	1	Fine.
	2	0	56,0	51,0	29,68		NE	1	Fine.
10	7	0	42,5	46,0	29,66		ESE	1	Fine.
	2	0	59,0	51,0	29,58		ESE	1	Rainy.
11	7	0	44,5	49,0	29,62	0,240	SSW	1	Cloudy.
	2	0	63,0	67,5	29,73		SE	1	Fine.
12	7	0	58,5	55,5	30,01		SE	1	Fine.
	2	0	72,0	64,0	30,02		NW	1	Cloudy.
13	7	0	62,0	61,0	29,91		NW	1	Fine.
	2	0	73,5	69,5	29,92		ESE	1	Cloudy.
14	7	0	56,0	63,0	29,89	0,041	SSE	1	Fine.
	2	0	71,5	68,5	29,89		NNW	1	Cloudy.
15	7	0	56,5	51,5	29,93		NW	1	Fine.
	2	0	67,0	68,0	29,96		SE	1	Cloudy.
16	7	0	52,0	54,0	29,96		NE	1	Cloudy.
	2	0	57,0	61,0	30,00				

METE-

METEOROLOGICAL JOURNAL

for May 1781.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			inches.	Inch.	Points.	Str.	
May 17	7	0	50,0	57,0	30,09	0,050	NE	1	Cloudy.
	2	0	57,0	59,5	30,08		NE	1	Cloudy.
18	7	0	48,5	53,5	29,95	0,114	ENE	1	Rainy.
	2	0	57,5	58,0	29,95		NE	1	Rainy.
19	7	0	55,0	56,0	29,93	0,076	NNW	1	Cloudy.
	2	0	60,5	60,5	29,93		ENE	1	Cloudy.
20	7	0	57,0	57,0	29,89	0,099	ENE	1	Cloudy.
	2	0	64,5	62,0	29,93		NE	1	Fine.
21	7	0	57,0	57,0	30,05		NE	1	Fine.
	2	0	69,0	67,0	30,08		E by N	1	Fine.
22	7	0	50,0	54,0	30,29		NE	1	Cloudy.
	2	0	57,5	58,5	30,29		NE	2	Fine.
23	7	0	57,0	48,0	30,35		NE	2	Fine.
	2	0	58,0	57,0	30,35		NE	2	Fine.
24	7	0	49,0	49,0	30,36		NE	2	Fine.
	2	0	56,0	55,0	30,34		SE	1	Fine.
25	7	0	51,5	50,0	30,23		SW	1	Fine.
	2	0	61,0	58,0	30,22		SW	1	Fine.
26	7	0	50,5	52,0	30,16		ESE	1	Fine.
	2	0	65,0	61,0	30,14		ESE	1	Fine.
27	7	0	48,5	51,0	30,15		ESE	1	Fine.
	2	0	70,0	63,0	30,15		S by W	1	Fine.
28	7	0	53,0	56,5	30,22		SW	1	Fine.
	2	0	72,0	63,5	30,22		SW	1	Fine.
29	7	0	55,5	57,5	30,26		ESE	1	Fine.
	2	0	76,0	70,0	30,16		SE	1	Fine.
30	7	0	60,5	65,0	29,98		SE	1	Fine.
	2	0	80,5	71,5	29,96		SE	1	Fine.
31	7	0	63,0	69,0	30,12		SE	1	Fine.
	2	0	78,0	76,0	30,04		SE	1	Fine.

METEOROLOGICAL JOURNAL

for June 1781.

	Time.	Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
June 1	7 0	65,0	66,0	30,04		NE	1	Fine.
	2 0	83,5	78,5	30,03		SW	1	Fine.
2	7 0	67,0	71,0	29,98		SW	1	Fine.
	2 0	84,0	78,0	29,96		SW	1	Fine.
3	7 0	60,0	65,0	29,87		SW	1	Rainy.
	2 0	75,0	74,0	29,80		SSW	2	Showery.
4	7 0	58,5	67,5	29,82	0,169	WSW	2	Cloudy.
	2 0	70 5	70,0	29,79		SW	2	Showery.
5	7 0	60,0	55,0	29,82		S by W	1	Fair.
	2 0	75,0	68,0	29,69		SW	1	Fine.
6	7 0	59,5	56,0	29,60	0,226	SSE	1	Cloudy.
	2 0	68,0	68,5	29,54		W by S	1	Fine.
7	7 0	58,5	65,0	29,55		SSW	1	Cloudy.
	2 0	64,5	67,0	29,55		SW	1	Fair.
8	7 0	53,5	60,0	29,49	0,055	SW	1	Rainy.
	2 0	60,0	62,0	29,42		SW	1	Showery.
9	7 0	57,0	60,0	29,68		SW	1	Fine.
	2 0	64,0	64,0	29,72		SW	1	Showery.
10	7 0	58,5	59,5	29,72	0,064	E by N	1	Fine.
	2 0	60,0	65 0	29,79		SE	1	Showery.
11	7 0	59,5	61,0	29,84		SSW	1	Cloudy.
	2 0	70,5	67,0	29,79		SSE	1	Cloudy.
12	7 0	56,0	63,0	29,82	0,057	NE	1	Rainy.
	2 0	67,0	66,5	29,78		NE	1	Fine.
13	7 0	59,5	64,0	29,77		SE	1	Cloudy.
	2 0	73,5	68,5	29,74		SSE	1	Fine.
14	7 0	63,0	65,0	29,71		SSE	1	Cloudy.
	2 0	66,0	67,0	29,66		SW	1	Fair.
15	7 0	59,5	61,0	29,79		SSE	1	Fine.
	2 0	75,0	68,5	29,65		SSE	1	Fine.
16	7 0	61,0	61,5	29,78		SSW	1	Fine.
	2 0	70,5	69.0	29,78		SSE	1	Fine.

METEOROLOGICAL JOURNAL

for June 1781.

	Time. H.	M.	Therm without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
June 17	7	0	61,5	65,0	29,84		SW	1	Fine.
	2	0	71,0	68,5	29,84		SSW	1	Fair.
18	7	0	65,5	66,0	29,91		SSE	1	Cloudy.
	2	0	73,5	70,5	29,95		SSE	1	Fair.
19	7	0	64,5	67,0	29,91	0,019	ESE	1	Rainy.
	2	0	75,0	72,5	29,84		E by S	1	S how.
20	7	0	70,0	71,5	29,82	0,098	NE	1	Fine.
	2	0	80,0	79,5	29,84		ESE	1	Fine.
21	7	0	68,0	70,5	29,79		ESE	1	Fine.
	2	0	79,0	76,0	29,70		ESE	1	Fair.
22	7	0	57,5	69,0	29,76		NE	1	Cloudy.
	2	0	75,0	73,0	29,74		NE	1	Fine.
23	7	0	57,0	64,0	29,82		NE	1	Cloudy.
	2	0	62,5	65,0	29,83		N by E	1	Rainy.
24	7	0	55,0	69,0	29,87		NE	1	Cloudy.
	2	0	67,0	66,0	29,85		NE	1	Fair.
25	7	0	55,0	61,0	29,82		NE	1	Cloudy.
	2	0	63,0	64,0	29,80		SSE	1	Cloudy.
26	7	0	59,0	62,0	29,93		NW	1	Cloudy.
	2	0	70,0	68,0	29,97		NW	1	Fine.
27	7	0	60,0	60,0	30,05		NW	1	Fine.
	2	0	73,0	70,0	30,05		NW	1	Fine.
28	7	0	63,5	66,5	30,14		NW	1	Fine.
	2	0	74,0	71,0	30,19		NNW	1	Fine.
29	7	0	59,0	59,0	30,42		NW	1	Fine.
	2	0	77,0	71,5	30,39		NW	1	Fine.
30	7	0	66,0	69,0	30,24		SW	1	Fine.
	2	0	81,0	78,0	30,24		WSW	1	Fine.

METEOROLOGICAL JOURNAL

for July 1781.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	65,0	67,0	30,04		SSE.	1	Fine.
	2	0	85,0	75,0	29,90		SSW	1	Fine.
2	7	0	74,0	75 0	29,69		SSW	1	Fine.
	2	0	76,0	76,0	29.77		SSW	1	Fine.
3	7	0	65 0	69,0	29,82		SSW	1	Cloudy.
	2	0	74,0	73 0	29,85		SSW	1	Fine.
4	7	0	63,0	65,0	30,04		SSW	1	Fine.
	2	0	71,5	71,0	30,11		WSW	2	Fine.
5	7	0	61,0	62,0	30,25		SSE	1	Fine.
	2	0	74,5	71,5	30,22		SW	1	Fine.
6	7	0	65,0	65,0	30,03		SE	1	Cloudy.
	2	0	78,0	74,5	29.95		SE	1	Fine.
7	7	0	62,0	68,0	29.81	0,050	SE	1	Cloudy.
	2	0	73,0	71,0	29,78		SE	1	Cloudy.
8	7	0	60,0	61,0	29,71	0,132	SE	2	Cloudy.
	2	0	65,5	67,5	29,74		S by W	2	Rainy.
9	7	0	60,5	66,0	29,92	0,293	SW	1	Rainy.
	2	0	68,5	68,0	29,92		NW	2	Cloudy.
10	7	0	68,0	65,0	29,97	0,114	SSW	1	Rainy.
	2	0	69,0	67,0	29,88		SW	1	Cloudy.
11	7	0	59,0	60,5	29.86	0,243	WSW	1	Cloudy.
	2	0	68,0	67,5	29,93		NW	1	Cloudy.
12	7	0	64,5	67,0	30,07		SW	1	Cloudy.
	2	0	76,5	71,0	30,13		SW	1	Fine.
13	7	0	67,0	70,0	30,16		SW	1	Fine.
	2	0	74,5	73,0	30,16		SW	2	Cloudy.
14	7	0	62,0	67,0	30,03		SW	1	Cloudy.
	2	0	66,5	70,0	30,01		WSW	1	Rainy.
15	7	0	57,0	66,0	30,11		WSW	1	Fine.
	2	0	68,0	67,5	30,13		W by S	1	Fine.
16	7	0	58,5	64,0	30,29		WSW	1	Fine.
	2	0	73,0	69,0	30,33		NW	1	Fine.

METEOROLOGICAL JOURNAL
for July 1781.

	Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
July 17	7	0	59,0	53,5	30,33		NW	1	Fine.
	2	0	73,0	69,0	30,29		NW	1	Fine.
18	7	0	62,5	67,0	30,26		SW	1	Fine.
	2	0	73,5	70,5	30,24		SW	1	Fine.
19	7	0	62,0	66,0	30,34		NW	1	Fine.
	2	0	70,0	69,0	30,35		NW	1	Fine.
20	7	0	62,0	65,0	30,41		NW	1	Fine.
	2	0	69,0	68,5	30,41		NW	1	Fine.
21	7	0	61,0	64,0	30,44		ESE	1	Fine.
	2	0	72,0	70,0	30,41		NW	1	Fine.
22	7	0	61,0	63,0	30,33		NW	1	Fine.
	2	0	75,0	72,0	30,23		NW	1	Fine.
23	7	0	62,0	65,0	30,17		NNW	1	Cloudy.
	2	0	72,5	71,0	30,15		NW	1	Fine.
24	7	0	64,0	69,0	30,07		SSW	1	Cloudy.
	2	0	79,0	75,0	30,03		WSW	1	Fine.
25	7	0	65,0	70,0	29,96		WSW	1	Fine.
	2	0	77,5	74,0	29,93		SSW	1	Fine.
26	7	0	60,0	65,0	29,89		SW	1	Cloudy.
	2	0	71,0	70,0	30,01		SW	1	Cloudy.
27	7	0	59,0	67,0	30,06		NNW	1	Cloudy.
	2	0	73,0	70,0	30,06		NNW	1	Cloudy.
28	7	0	73,0	67,0	30,05		NW	1	Fine.
	2	0	75,0	71,0	30,01		SW	1	Fine.
29	7	0	66,0	68,0	30,01	0,213	SW	1	Cloudy.
	2	0	76,0	73,0	30,08		SW	1	Fine.
30	7	0	68,0	71,0	30,19		SW	1	Cloudy.
	2	0	80,0	76,0	30,17		SW	1	Fine.
31	7	0	67,0	70,0	30,06		SE	1	Fine.
	2	0	84,0	80,0	30,05		SE	1	Fine.

METEOROLOGICAL JOURNAL

for August 1781.

	Time. H.	Time. M.	Therm. without	Therm. within.	Barom.	Rain.	Winds. Points.	Winds. Str.	Weather.
			°	°	Inches.	Inch.			
Aug. 1	7	0	68,5	71,0	29,96		WSW	1	Cloudy.
	2	0	78,0	75,0	30,01		SW	1	Fine.
2	7	0	63,0	65,0	30,12		SW	1	Fine.
	2	0	71,5	69,5	30,11		WSW	1	Fine.
3	7	0	63,0	64,0	30,27		SW	1	Fine.
	2	0	73,0	70,0	30,28		SW	1	Fine.
4	7	0	63,0	65,0	30,39		SW	1	Cloudy.
	2	0	73,0	70,0	30,37		NE	1	Fine.
5	7	0	62,0	64,0	30,33		NE	1	Fine.
	2	0	72,0	70,0	30,24		NE	1	Fine.
6	7	0	63,0	65,5	30,07		SSE	1	Cloudy.
	2	0	77,0	72,0	30,07		SE	1	Fine.
7	7	0	63,0	66,0	30,08		SE	1	Cloudy.
	2	0	77,0	72,0	30,07		SE	1	Fine.
8	7	0	62,0	68,0	29,94	0,026	WSW	1	Cloudy.
	2	0	66,0	68,5	29,91		SW	1	Rainy.
9	7	0	63,0	67,0	29,95	0,684	SSW	1	Fair.
	2	0	78,0	71,5	29,98		SSW	1	Fine.
10	7	0	68,5	70,5	29,90	0,042	SSW	1	Cloudy.
	2	0	80,0	76,0	30,03		SSW	1	Cloudy.
11	7	0	63,0	71,5	30,06		SSW	1	Fine.
	2	0	82,0	77,0	30,05		SE	1	Fine.
12	7	0	68,0	71,0	30,02		SSW	1	Fine.
	2	0	81,5	78,0	30,01		SW	1	Fine.
13	7	0	67,0	72,0	29,93		SW	1	Rainy.
	2	0	80,5	75,5	29,87		SW	1	Fine.
14	7	0	63,0	72,5	29,93		SW	1	Fine.
	2	0	75,0	74,0	29,85		SW	1	Cloudy.
15	7	0	58,0	68,5	29,65	0,271	SSW	2	Fair.
	2	0	65,5	70,0	29,59		SSW	2	Rainy.
16	7	0	61,0	67,5	29,62	0,067	SW	1	Fine.
	2	0	72,0	69,5	29,61		SSW	1	Fair.

METEOROLOGICAL JOURNAL
for Auguſt 1781.

	Time.	Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
				Inches.	Inch.	Points.	Str.	
Aug. 17	7 o	60,5	65,5	29,74	0,182	SW	1	Fine.
	2 o	72,0	68,0	29,83		SW	1	Fine.
18	7 o	57,0	64,0	29,89	0,087	SW	1	Fine.
	2 o	73,0	68,5	29,86		SSW	1	Fine.
19	7 o	58,0	64,5	29,73		NNE	1	Rainy.
	2 o	59,0	65,0	29,66		N by W	1	Rainy.
20	7 o	53,0	62,5	29,77	0,147	NW	1	Fine.
	2 o	67,0	66,0	29,82		NW	1	Cloudy.
21	7 o	55,0	62,0	30,11		NW	1	Fine.
	2 o	68,0	66,0	30,15		NNW	1	Fine.
22	7 o	52,0	61,0	30,25		NE	1	Fine.
	2 o	71,0	69,5	30,27		NE	1	Fine.
23	7 o	60,0	65,0	30,04		SSW	1	Cloudy.
	2 o	80,0	70,0	29,96		SW	1	Fine.
24	7 o	63,0	67,0	29,76		SE	1	Cloudy.
	2 o	75,5	71,0	29,66		SSE	1	Fine.
25	7 o	61,5	68,5	29,54	0,302	WSW	1	Fine.
	2 o	72,0	71,0	29,67		SW	1	Cloudy.
26	7 o	61,5	68,0	29,93		SW	1	Fine.
	2 o	75,5	73,0	29,96		SW	1	Cloudy.
27	7 o	68,0	69,5	29,82		SSE	1	Cloudy.
	2 o	79,0	76,0	29,75		SSW	1	Fine.
28	7 o	67,5	70,5	29,66		SSW	1	Cloudy.
	2 o	65,0	72,5	29,58		SW	1	Rainy.
29	7 o	64,0	70,0	29,59		SW	2	Fine.
	2 o	72,5	71,0	29,77		SW	2	Fine.
30	7 o	64,0	68,0	30,01		SW	1	Fine.
	2 o	74,0	70,0	29,98		SSE	1	Cloudy.
31	7 o	66,0	70,0	29,72	0,390	SE	1	Rainy.
	2 o	75,0	73,0	29,88		SE	1	Fine.

1781	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Inches.
Jan.	51,5	25 0	38,5	49,0	32,0	35,9	30,55	29,04	29,90	1,348
Feb.	52,5	31,0	42,8	51,0	36,0	45,7	30,34	28,95	29,65	1,676
Mar.	60,5	32,0	44,8	53,5	35,0	45,9	30,47	29,88	30,21	0,292
Apr.	66,5	37,0	49,2	67,0	38,0	52,8	30,34	29,29	29,88	0,650
May	80,5	42,0	56,8	76,0	44,0	56,8	30,36	29,58	30,05	0,619
June	84,0	53,5	66,2	79,5	55,0	66,8	30,42	29.42	29,84	0,688
July	84,0	57,0	68,4	80,0	53,5	68,7	30,44	29,69	30,05	1,045
Aug.	82.0	52,0	67,7	78 0	61,0	69,2	30,39	29,54	29,95	2,198
Mean of 8 mon.			53,0			55,2			29,95	8,516

APPENDIX.

I. *Account of a new Kind of Rain. Written by the Count* de Gioeni, *an Inhabitant of the 3d Region of Mount Etna; communicated by Sir* William Hamilton, *K. B. F. R. S.* See p. 1.

Volat per Mare magnum cinis decoctus, et terrenis nubibus excitatis, tranſmarinas quoque provincias pulvereis guttis implevit. CASSIOD. lib. IV. var. epiſt. 50.

THE morning of the 24th inſtant there appeared here a moſt ſingular phenomenon. Every place, expoſed to the air, was found wet with a coloured cretaceous grey water, which, after evaporating and filtrating away, left every place covered with it to the height of two or three lines; and all the iron-work that was touched by it became ruſty.

The public, inclined to the marvellous, fancied various cauſes of this rain, and began to fear for the animals and vegetables.

In places where rain water was uſed, they abſtained from it: ſome ſuſpecting vitriolic principles to be mixed with it, and others predicting ſome epidemical diſorder.

Thoſe who had obſerved the exploſions of Etna twenty days and more before, were inclined to believe it originated from one of them.

The ſhower extended from N. ¼ N.E. to S. ¼ SW. over the fields, about ſeventy miles in a right line from the vertex of Etna.

There is nothing new in volcano's having thrown up ſand *, and alſo ſtones †, by the violent expanſive force generated within them, which ſand has been carried by the wind to diſtant regions.

But the colour and ſubtilty of the matter occaſioned doubts concerning its origin; which increaſed from the remarkable circumſtance of the water in which it came incorporated ‡; for which reaſons ſome other principle or origin was ſuſpected.

It became, therefore, neceſſary by all means to aſcertain the nature of this matter, in order to be convinced of its origin, and of the effects it might produce. This could not be done without the help of a chemical analyſis. To do this then with certainty, I endeavoured to collect this rain from places where it was moſt probable no heterogeneous matter would be mixed with it. I therefore choſe the plant called *Braſſica Capitata*, which having large and turned-up leaves, they contained enough of this coloured water; many of theſe I emptied into a veſſel, and left the contents to ſettle till the water became clear.

This being ſeparated into another veſſel, I tried it with vegetable alkaline liquors and mineral acids; but could obſerve no decompoſition by either. I then evaporated the water in order to reunite the ſubſtances that might be in ſolution; and

* The authority of CASSIOD. prefixed to this account is ſtrengthened by SENECA, in his 2 *lib. de Queſt. Nat.*

Ætna aliquando multo igne abundavit, ingentem vim arenæ ardentis effudit, involutus eſt dies pulvere, populoſque ſubita nox terruit.

But without having recourſe to the numerous old accounts of this volcano, and of Veſuvius, we have, within theſe twenty years, ſeen many of thoſe rains in Sicily originating in Etna; and the laſt, preceding the eruption of laſt year, was compoſed of little fragments of bituminous pumice ſtone, or *ſtumie*.

† The ſtone, deſcribed by PLINY, which fell in Thrace, the ſhower of ſtones on mount Albano after the ruin of Alba, which LIVY mentions, and many others of like nature, remarked by the ancients as miraculous rains, have been diſcovered to be volcanic. As to Etna we have, in our days, ſeen new mountains formed by the ſtones, or rather the lava; and as to the ancients, beſides STRABO and many others, the poet PINDAR writes, that *aliquando non tantum rivos ignes ejecit, ſed ſaxa ignita.* PIND. ap Brit. lib. V. c. 14 p. 2.

‡ In many of the writers on Etna, ſhowers of ſand, or other productions, mixed with water, are not to be found.

touching it again with the aforesaid liquors, it shewed a slight effervescence with the acids. When tried with the syrup of violets, this became a pale green; so that I was persuaded it contained a calcareous salt *. With the decoction of galls no precipitation was produced.

The matter being afterwards dried in the shade, it appeared a very subtile, fine earth, of a cretaceous colour, but inert, from having been diluted by the rain.

I next thought of calcining it with a slow fire, and it assumed the colour of a brick. A portion of this being put into a crucible, I applied to it a stronger heat, by which it lost almost all its acquired colour. Again, I exposed a portion of this for a longer time to a very violent heat (from which a vitrification might be expected); it remained however quite soft, and was easily bruised, but returned to its original dusky colour.

From the most accurate observations of the smoke from the three calcinations, I could not discover either colour or smell that indicated any arsenical or sulphureous mixture.

Having therefore calcined this matter in three portions, with three different degrees of fire, I presented a good magnet to each; it did not act either on the first or second; a slight attraction was visible in many places on the third: this persuaded me, that this earth contains a martial principle in a metallic form, and not in a vitriolic substance †.

The nature of these substances then being discovered, their volcanic origin appears; for iron, the more it is exposed to violent calcination, the more it is divided, by the loss of its phlogistic principle; which cannot naturally happen but in the great chimney of a volcano. Calcareous salt, being a marine salt combined with a calcareous substance by means

* Tried likewise with a solution of lead in the vegetable acid, it lost its natural colour and its transparency, and became milky. I should be inclined to believe this to be the effect of the alkaline particles, and thus account for the efflorescence on the iron's being exposed to the air.

† Because, otherwise the water would not have produced an effervescence with the acids, but would have shewn it with the alkalies; and, in the triple calcination, the red colour would rather have been increased than diminished.

of violent heat *, cannot be otherwiſe compoſed than in a volcano †.

As to their dreaded effects on animals and vegetables, every one knows the advantageous uſe, in medicine, both of the one and the other, and this in the ſame form as they are thus prepared in the great laboratory of nature.

Vegetables, even in flower, do not appear in the leaſt macerated, which has formerly happened from only ſhowers of ſand ‡.

How this volcanic production came to be mixed with water may be conceived in various ways.

Etna, about its middle regions, is generally ſurrounded with clouds that do not always riſe above its ſummit, which is 2900 paces § above the level of the ſea. This matter being thrown out, and deſcending upon the clouds below it, may happen to mix and fall in rain with them in the uſual way. It may alſo be conjectured, that the thick ſmoke which the volcanic matter contained might, by its rarefaction, be carried in the atmoſphere by the winds, over that tract of country ‖; and then, cooling ſo as to condenſe and become ſpecifically

* The burning of lime-ſtone may indeed produce the compoſition from whence reſults the calcareous ſalt: but it is evident, that ſuch a quantity could only proceed from a volcano.

† Many and repeated experiments on the produce of Etna have perſuaded me, that marine ſalt is one of the chief and moſt abundant *menſtrua* which excite the effervescence of a volcano, or that it is the baſis of it (as a friend of great knowledge has lately made me believe). I find calcareous ſalt in the old lava, and common ſalt ſublimated to ammoniac in the fiſſures and openings of the new irruptions. But this is not the place for that which requires a larger volume. I may, perhaps, ſay more of it on another occaſion.

‡ I have repeatedly obſerved, that the ſand-ſhowers of our mountain are moſtly compoſed of calcined matter, and of little cryſtals of ſchorl, with a ſmall portion of arſenical and ſometimes ſaline ſulphureous particles, which unites the ſchorl to the other ſubſtances, ſo that the particles or grains are thereby enlarged. Sometimes the rain falls to the ground ſtill warm.

§ The meaſure of the height of the mountain has twice come out to be thus deſcribed; not, however, that I give it for certain, well knowing that altimetry requires exact inſtruments and repeated obſervations. I mean to try it with the barometer, when convenient.

‖ That this hypotheſis may not appear exaggerated as to the quantity of ſmoke that

ſpecifically heavier than the air, might deſcend in that coloured rain.

I muſt, however, leave to philoſophers (to whom the knowledge of natural agents belongs) the examination and explanation of ſuch phenomena, confining myſelf to obſervation and chemical experiments *.

P. S. On Friday the 4th of May, about a quarter paſt three in the afternoon, a ſlight ſhock of an earthquake was felt in the country about Etna, which became more ſenſible at ſome diſtance from the mountain: its direction was from north to ſouth. The volcano had continued its flames and exploſions; and the night before, a column of ſmoke, compoſed of globes as it were piled upon each other, had aſcended over the crater to double the height of the mountain, as far at leaſt as one could judge at the diſtance of twenty-two miles, which the vertex is in a right line from this city. This remained the whole night perpendicular, only one of the globes had ſeparated and lengthened out to the weſtward from the ſummit. Now and then all the inſide of the column, and of the lengthened out-part, became illuminated by electric fire, which was of a deep red colour, and gradually went out again, beginning at the bottom, in about two ſeconds.

that muſt be ſuppoſed, I ſhall mention what was obſerved by CICERO: *Cratere flamma erumpit, fumo mixta tam copioſo, ut, dum Boreas ſpirat, Melitam uſque per aera illum ſublimem propellat ad* IX. *millia paſſuum ſpatium.* CIC. de Nat. Deor. lib. II.

* WALLERIUS (in his Mineral. vol. II. Hidrol. § 5.) ſays: *La Phyſique eſt plus univerſelle dans ſes vues, et plus philoſophique dans ſon examen, le phyſicien enviſage, raiſonne, explique, le naturaliſte regarde, ramaſſe, et range; celui-ci vous dira il exiſte tel corps dans la nature, il eſt fait, ſoit au dedans, ſoit au dehors de telle ou telle maniere, il eſt de tel ou tel regne, claſſe, ordre, eſpéce, variété; celui la pretendra vous expliquer les cauſes de ſon exiſtence, de ſes formes, et de ſes propriétés.*

The illuſtrious LINNÆUS, in Anal. Tranſalp. anno 1740, § 2. ſays thus: *Phyſica eſt ſcientia de qualitatibus elementorum, hiſtoria naturalis autem circa cognitionem corporum naturalium verſatur.* The true naturaliſt ought to be learned both in phyſics and in chemiſtry; but ſtill we know not where the diviſion between the two ſciences is.

The fire has continued on the crater till this day, May 8th, ejecting red-hot maſſes or ſtones, which rolling beautifully down the cone, have illuminated this region; ſome lava has run over from the crater towards the W.N.W. but without having force enough to burſt the ſides or walls of the volcano; ſo that we may apply the hiſtorical paſſage, MARCO ÆMILIO & AURELIO Coſſ. *Ætna mons terræmotu, ignes ſuper verticem late diffudit.* Jul. Obſequ. de Prodig. c. 89.

II. *Of the Method of rendering very ſenſible the weakeſt Natural or Artificial Electricity. By M.* Alexander Volta, *Profeſſor of Experimental Philoſophy in* Como, *&c. &c.; communicated by the Right Hon.* George *Earl* Cowper, *F. R. S.* See p. 237.

PART I.

1. IT will be readily allowed, that an apparatus capable of rendering perceptible, or, as it were, of magnifying the ſmalleſt, and otherwiſe unobſervable, degrees of natural as well as artificial electricity, is of great advantage to the ſcience of electricity in general, and eſpecially for the inveſtigation of atmoſpherical electricity, which by this means may be rendered very ſenſible and conſpicuous when it is not to be diſcovered by common atmoſpherical conductors. This method is founded upon a particular uſe of my *electrophorus*, which is a machine well known to electricians.

2. Whenever in obſerving the atmoſpherical electricity no degree of it can be diſcovered by the ordinary methods of performing thoſe experiments, it is difficult to determine whether any electricity at all does or does not exiſt in the atmoſphere at thoſe times; ſince it may exiſt, and the quantity of it only be ſo ſmall as not to affect the electrometers employed. An ordinary conductor, erected in the beſt manner for the purpoſe of obſerving the atmoſpherical electricity, when the ſky is free from electrical clouds, ſeldom or never ſhews any ſigns of electricity. In that caſe, therefore, if we rely upon the common electrometers, even the moſt ſenſible, we muſt conclude, that neither the conductor nor the atmoſphere, ſo high as the conductor reaches, contains any electricity: but by means of the apparatus I am going to deſcribe, it will be found, that the ſaid conductors are never entirely void of electricity,

tricity, and it muſt be conſequently concluded, that the air, which ſurrounds them, is alſo at all times electrified. This method not only ſhews the exiſtence of electricity, but gives alſo room to aſcertain whether it is poſitive or negative, and that when the atmoſpherical conductor itſelf is not capable of attracting the fineſt thread; but if the conductor were to ſhew any very ſmall attraction, then, by means of our apparatus, there may be obtained even ſtrong ſparks.

3. The electrophorus in this caſe might perhaps better deſerve the name of *electrometer*, or *micro-electrometer*, but I had rather call it a *condenſer of electricity*, for the ſake of uſing a word which expreſſes at once the reaſon and cauſe of the phenomena to be treated of in this paper, as will be made evident in the ſecond part.

The whole method may be reduced to the following few obſervations. I. An electrophorus muſt be procured, the reſinous coat of which muſt be very thin, and either not at all electrified, or, if electrified, its electricity be entirely extinguiſhed.

II. Its uſual metal plate muſt be laid upon this reſinous and unelectrified plate, in full and flat contact; but care muſt be taken that it does in no point touch the lamina of metal upon which the reſinous ſtratum is uſually faſtened.

III. Thoſe plates being ſo conjointly placed, a conducting communication, *viz.* a wire muſt be brought from the atmoſpherical conductor to touch the metal plate of the electrophorus, and to touch that only.

IV. The apparatus muſt be left in that ſituation for a certain time, *viz.* till the metal plate may have acquired a ſufficient quantity of electricity through the conducting communication, which brings it from the atmoſpherical conductor very ſlowly.

V. Laſtly, the conducting communication muſt be removed from the contact of the metal plate: the metal plate is then ſeparated from the reſinous one, by lifting it up by its inſulating handle, after which it is in a ſtate of attracting, of electrifying an electrometer, or, if the electricity is ſufficiently ſtrong, of giving ſparks, &c. at the ſame time the atmoſpherical

conductor

conductor itſelf ſhews either no electricity at all, or exceeding ſmall ſigns of it.

4. It was mentioned above (IV.) that the conducting wire muſt be left in contact with the metal plate *for a certain time*, the length of which, however, is not eaſily determined, ſince it depends upon variable circumſtances. When the conductor itſelf ſhews no ſigns of electricity, then it will be neceſſary to leave the apparatus, as directed above, during eight, ten, or more minutes. But if the conductor itſelf is capable of juſt attracting a very ſmall thread, then it will be ſufficient to leave the apparatus in contact as above mentioned, for a few ſeconds only, in order afterwards to obtain from it very conſpicuous electrical appearances.

5. Reſpecting the conducting communication between the atmoſpherical conductor and the metal plate, care ſhould be taken that it be made of the feweſt joints poſſible, or rather of one piece, ſince the difficulty of tranſmitting very ſmall quantities of electricity is conſiderably increaſed by every interruption, and it may thereby be quite obſtructed, as is often the caſe when a chain is uſed for that purpoſe.

6. As for the electrophorus to be uſed, it muſt be farther remarked, firſt, that its being very thin, as mentioned above, is of great importance; it having been obſerved, that the thinner the reſinous ſtratum is, the greater quantity of electricity can be accumulated into the metal plate laid upon it; which is the caſe whether the electricity is brought to it from the atmoſphere, as in the abovementioned inſtance, or from any other electric power. The thickneſs of one-fiftieth of an inch, or that of a common coat of varniſh, is very proper; whereas if the reſin was an inch thick or more, the experiments would anſwer very badly.

7. Secondly, the ſurface of the reſinous ſtratum, as well as the under ſurface of the metal plate, muſt be as plain and as ſmooth as poſſible, in order that the two ſurfaces may coincide more perfectly when laid one upon the other. It is well known how much this circumſtance favours the effect of the electrophorus; for this reaſon, in my publication on that inſtrument,

I recommended

I recommended it as a thing eſſential to obſerve *: but this circumſtance is ſtill more eſſential when the ſame apparatus is to ſerve as a *condenſer* of electricity.

8. Laſtly, it deſerves to be repeatedly and particularly obſerved, that the reſinous plate, when it is to be uſed for our experiment, ſhould be quite free from any the leaſt electricity, otherwiſe the experiments cannot be depended upon. If, therefore, the reſinous plate has been excited before, ſo as to remain in ſome meaſure electrified, all poſſible care ſhould be taken to deprive it of that electricity, which however is not eaſily done. The moſt effectual method of doing it to expoſe the reſinous plate to the hot rays of the ſun or to the fire, ſo that its ſurface may be ſlightly melted, by which means it will entirely loſe its electricity †. The flame of a candle, or of a piece of paper, will eaſily deprive the reſin of its electricity, if its ſurface be paſſed over the flame. In order to obſerve whether the reſinous plate is quite free from any electricity, the metal plate muſt be laid upon it, there it muſt be touched with a finger, and afterwards, being lifted up after the uſual manner, it muſt be preſented to a fine hair; for if the hair is not attracted, you may conclude, that the reſinous plate has no electricity, and conſequently the apparatus is fit to be uſed as a *condenſer* of electricity.

9. Were I aſked, to what degree the electricity might be condenſed, or how much the electrical phenomena could be

* See the two letters addreſſed to Dr. PRIESTLEY, and publiſhed in the *Scelta d'Opuſculi intereſſanti* of Milan for the year 1775.

† It has been believed for a long time, that to heat, and eſpecially to melt, ſulphur and reſins, was ſufficient to excite in them ſome electricity; but except the tourmalin and ſome other ſtones, which are really excited by heat alone, the reſins and ſulphur never become electrified by that means, except when they have by ſome means or other ſuffered any friction. The miſtake, as Father BECCARIA obſerved, was occaſioned by this, *viz.* that even the leaſt friction of the hand, or other body, is ſufficient to excite ſuch ſubſtances in thoſe favourable circumſtances; without which friction, thoſe ſubſtances, melted and left to cool by themſelves, are ſo far from acquiring any electricity, that they loſe every veſtige of it in caſe they were excited before the fuſion, as may be eaſily proved by experiment: nor ought this to appear wonderful, ſince fuſion or a ſtrong degree of heat renders every body a conductor of electricity.

increaſed

increaſed by this apparatus; I would anſwer, that it is not eaſy to be determined, as it depends upon various circumſtances; however, *cæteris paribus*, the augmentation is greater in proportion as the body which ſupplies the metal plate with the electricity has a greater capacity, and is larger in proportion as the electricity is weaker. Thus we obſerved above (§ 2. and following) that if the atmoſpherical conductor has barely power enough to attract a very fine thread, it is nevertheleſs capable of infuſing ſuch a quantity of electricity into the metal plate of the electrophorus, as to let it not only actuate an electrometer, but even dart ſtrong ſparks. But if the electricity of the atmoſpherical conductor is ſo ſtrong as to afford ſome ſparks, or to let the index of the electrometer riſe to five or ſix degrees, then the metal plate of the electrophorus, which receives the electricity from this conductor, according to our method, will certainly let the index of the electrophorus riſe to the higheſt degree, and will give a ſtronger ſpark, yet it may be plainly perceived, that the condenſation is proportionably leſs in this than in the other caſe. The reaſon is, becauſe the electricity cannot be accumulated beyond the greateſt degree, *viz.* when the electricity is increaſed ſo much as to be diſſipated every way. Therefore, according as the electric power, which ſupplies the condenſer, is neareſt to the higheſt degree, the condenſation is proportionably leſs: but in that caſe there is no need of a condenſer, ſince its principal uſe is to collect and render ſenſible that ſmall quantity of electricity, which would otherwiſe remain imperceptible and unobſerved.

10. Whenever, therefore, the atmoſpherical conductor by itſelf gives ſufficiently ſtrong ſigns of electricity, then there is no occaſion to uſe our condenſing apparatus. Beſides, when the electricity is ſtrong, it often happens, that part of the electricity of the metal plate is impreſſed upon the reſin, in which caſe the apparatus acts as an electrophorus, and conſequently is unfit for our purpoſe (§ 8.).

11. In order to avoid ſuch an inconvenience, I have thought of ſubſtituting to the reſinous plate a plane, which ſhould not be a perfect electric, or quite impervious to electricity, but which ſhould be an imperfect conductor, ſuch as might hinder, in a cer-

tain meaſure only, the free paſſage of the electric fluid through its ſubſtance. There are many conductors of this kind; as, for inſtance, a clean and dry marble ſlab, a plate of wood (likewiſe clean and very dry, or covered with a coat of varniſh, or wax) and the like. The ſurface of thoſe bodies does not contract any electricity, or if any electricity adheres to them, it vaniſhes ſoon, on account of their ſemi-conducting nature; for which reaſon they cannot anſwer the office of an electrophorus, and therefore are more fit to be uſed as condenſers of electricity.

12. Beſides the advantages above mentioned, there is another, which ariſes from ſubſtituting an imperfectly conducting plane to the reſinous plate, namely, that the metal plate laid upon one of theſe does actually condenſe or acquire a greater quantity of electricity than when laid upon the reſinous plate, or other perfect electric; for ſince, as was ſaid above, § 6. the thinner the reſinous ſtratum is, the better it anſwers our purpoſe; in the caſe of a varniſhed or waxed board, this ſtratum becomes exceedingly thin, and it becomes nothing when an imperfectly conducting ſubſtance is uſed, ſuch as a marble ſlab, a very dry piece of wood, &c.

13. On the other hand, care ſhould be taken, in chooſing the above mentioned plane, that it be not too much of a conducting nature, or capable of becoming ſo in a very ſhort time, it being quite neceſſary, that the electricity ſhould find a conſiderable degree of reſiſtance in going through its ſubſtance. In chooſing, or in preparing, ſuch a plane by drying, or otherwiſe, it is better to render it too near to than too far from the nature of a non-conductor. A marble ſlab, or a board properly dried, anſwers admirably well, and is preferable to any other plane: otherwiſe the reſinous plate of an electrophorus is preferable to a common table or marble ſlab not prepared; for theſe bodies, being in ſome meaſure imbibed with moiſture, conduct much better than is neceſſary.

14. To be more particular, I ſhall add, that for this purpoſe it is better to uſe a flat piece of marble, and to grind it againſt the metal plate, till they coincide ſo well as to ſhew a ſenſible coheſion between them. Afterwards the piece of marble ſhould be expoſed for ſeveral days to the heat

heat of a warmed place, ſuch as an oven, a chimney, &c. in order to expel the moiſture, and to render it quite fit for our experiments (§ 12. 13.). The marble, thus prepared, will continue dry for a conſiderable time, except it be long expoſed to very damp air. As for the ſmall quantity of moiſture which the marble may accidentally and ſuperficially attract, it may be removed by expoſing it to the ſun, or to a fire, or even by wiping it with a dry and clean cloth, previous to the performing of experiments.

15. It is not every ſort of marble that will anſwer equally for this purpoſe. The old marbles, which have been long preſerved in dry places, anſwer better than thoſe which have been recently dug from the quarry. The difference of the ſpecies of marble is alſo of conſequence in this buſineſs; I have found ſome marbles which, without any preparation, anſwer vaſtly well, whereas others will not do near ſo well, even when properly prepared; excepting, however, when they are preſerved hot during the experiment; for, in that caſe, they anſwer better than the beſt pieces of marble that are not preſerved hot. It is always advantageous to warm the marble previous to the experiment.

16. Inſtead of preparing the piece of marble by a long continued heat, it will be ſufficient to give it a coat of copal varniſh, or amber, or lac varniſh: after which it muſt be kept in an oven for a ſhort time. By this means even the worſt ſort of marble anſwers very well, even without previouſly warming or keeping it hot during the experiment.

17. By means of the varniſh even a metal plate may be uſed inſtead of the marble. This ſhould be firſt made flat by grinding it againſt the upper plate, and then it muſt be varniſhed, but rather thicker than when the varniſh is laid upon the marble. In this caſe both the plates might be varniſhed, though it is ſufficient to varniſh one of them.

18. Here it may be ſaid, that in fact we are returned to the electrophorus. This is true; and indeed the varniſhed metal, or marble, or wood, may be excited by a very ſlight friction, even ſometimes by the ſimple laying of the metal plate upon it,

eſpecially when they are hot; hence there is no occaſion to warm them, when they are good for the purpoſe, leſt they ſhould be ſo well prepared as to be eaſily excited, and then act like an electrophorus.

19. However, the advantages which a varniſhed plate has above the common electrophorus are, 1. that the varniſh is always thinner than the common reſinous ſtratum of an electrophorus. 2. That the varniſh acquires a more ſmooth and plain ſurface; hence the metal plate may be more eaſily, and to more advantage, adapted to it.

20. Inſtead of the above mentioned plane of marble or metal varniſhed, there may be ſubſtituted, with equal advantage, any ſort of plane covered with dry and clean oil-cloth or oil-ſilk or ſattin and other ſilk-ſtuff that is not conſiderably thick; which will anſwer very well, without requiring any more than perhaps a ſlight warming. The ſilk-ſtuffs anſwer better for this purpoſe than thoſe made of cotton or wool, and theſe better than linen. However, by a previous drying and keeping them hot during the experiment, paper, leather, wood, ivory, bone, and every ſort of imperfect conductor, may be made to anſwer to a certain degree.

21. If thoſe imperfectly conducting ſubſtances were dried too much, then they would become quite electrics, and conſequently uſeleſs for our purpoſe (as will be made appear better in the ſecond part of this paper), excepting when they were uſed like reſins, &c.

22. I muſt not omit to mention alſo, that the apparatus may be rendered more ſimple by applying the ſilk or other ſemi-conducting ſtratum to the upper, *viz.* to the metal plate, which is furniſhed with a glaſs handle inſtead of the marble or other plate, which in that caſe becomes uſeleſs: for in its ſtead a plane of any kind may be uſed, ſuch as a common wooden or marble table, even not very dry, a piece of metal, a book, or other conductor, whether perfect or imperfect, it being only neceſſary that its ſurface be flat.

In fact, nothing more is requiſite for our experiment than that the electricity, which tends to paſs from one ſurface to the other,

other, ſhould find ſome reſiſtance or oppoſition in either of the ſurfaces, as will be made more evident in the ſecond part. It is immaterial whether the non-conducting or ſemi-conducting ſtratum be laid upon one or the other of the planes, it being only neceſſary that they ſhould coincide very well together, which cannot be eaſily obtained when a common table is uſed for one of the planes, which is the only reaſon why it is better to uſe two planes which have been worked flat by grinding one upon the other, and one of them varniſhed, &c. A ſingle metal plate, covered with ſilk, with three ſilk ſtrings faſtened to it by way of a handle, may be conveniently uſed for ordinary experiments.

23. Hitherto we have conſidered the uſe of our condenſer in exploring the weak atmoſpherical electricity, which is brought down by the atmoſpherical conductor *. But this, though the principal, is not the only uſe to which it may be applied. It ſerves likewiſe to diſcover the artificial electricity when this is ſo weak as not to be diſcoverable by any other means, which happens in various caſes, ſome of which I ſhall now proceed to mention.

24. A Leyden phial charged, and then diſcharged by touching its coated ſides three or four times with the diſcharging rod, or the hand, ſeems to be quite deprived of electricity, yet

* Here it will be proper to mention a remarkable obſervation, which I have made on the atmoſpherical electricity with the help of the *condenſer*. The late Mr. CANTON and others affirmed that they had obtained ſtronger ſigns of electricity from their atmoſpherical apparatus at the time of an *aurora borealis*, than at other times; but various other philoſophers doubted of the influence of electricity in that meteor, and ſome abſolutely denied it. I myſelf was much in doubt about it; but at preſent Mr. CANTON's aſſertion ſeems to be eſtabliſhed beyond a doubt, as I have obſerved by actual experiment. During the ſtrong *aurora borealis*, which appeared in the night of the 28th of July, 1780, the light of which riſing gradually from the horizon, reached the zenith at near eleven o'clock, and enlightened the heavens with a reddiſh light, the weather being clear and windy; our condenſing apparatus being applied to an atmoſpherical conductor, gave fine bright ſparks; whereas, at other times, that is, in clear weather, and at every hour of the day or night, the ſame apparatus afforded either no ſparks at all, or exceedingly ſmall ones, the reaſon of which was becauſe the ſaid conductor was not much elevated.

if you touch with the knob of it the metal plate of our condenſer, when properly ſituated (*viz.* upon an imperfectly conducting plane, &c.) and immediately after take up the ſaid plate, this will be found to give very conſpicuous ſigns of electricity, which ſhews that the Leyden phial is not quite deprived of electricity as it appeared. But if the phial was left ſo far charged as juſt to attract a light thread, then if the metal plate were to be touched by the knob of it, even for a moment, it would afterwards, when lifted up, give a ſtrong ſpark, and if then it were to be touched again by the knob of the phial, it would afford a ſecond ſpark hardly ſmaller than the former, and thus ſpark after ſpark may be obtained for a long time, which is a very ſurprizing experiment.

This method of producing ſparks by means of a phial, which is not charged ſo high as to give ſparks of itſelf, is very convenient for various pleaſing experiments; as, for inſtance, that of lighting the inflammable air-piſtol, or lamp, contrived by me, eſpecially when a perſon is provided with one of thoſe phials, prepared after the manner recommended by Mr. TIBERIUS CAVALLO *, which when charged may be carried in the pocket for a long time. Thoſe phials, as they retain a ſenſible charge for ſeveral days, will retain an inſenſible one for weeks or months. I mean, by an inſenſible charge, ſuch as cannot be diſcovered but by the help of the condenſer, in which caſe it becomes more than ſenſible, and ſufficient for the experiment of the inflammable air-piſtol, &c.

25. Secondly, Suppoſe you have an electrical machine ſo badly in order that its conductor will not afford any ſpark, but will juſt attract a thread; then if you let this conductor touch the metal plate of the condenſer, and after ſuffering it to continue in that ſituation for a few minutes, whilſt the machine is kept in motion, lift up the metal plate, you will obtain from it a ſtrong ſpark.

26. Thirdly, In caſe the electrical machine acts very well, but its conductor is ſo badly inſulated, that it will not give any ſparks, as when the conductor touches the walls

* See his Treatiſe on Electricity.

of

of the room, or when a chain falls from it upon the table; then if you let the ſaid conductor in that ſtate touch the metal plate of the condenſer, whilſt the electrical machine is in action, the plate will afterwards give ſufficiently ſtrong ſigns of electricity, which ſhews the great power this apparatus has of drawing and condenſing the electricity.

27. Fourthly, The uſual way of rubbing divers bodies, and then preſenting them to an electrometer in order to examine their electricity, is often inſufficient, that is, it makes the experimenter believe, that a body has not acquired any electricity at all, only becauſe the quantity of it is too ſmall to affect an electrometer. In this caſe it is very advantageous to rub thoſe bodies with the metal plate of our apparatus, which plate for this purpoſe muſt be naked; for if the plate be afterwards preſented to an electrometer, this will be electrified conſiderably, however little electricity the rubbed bodies themſelves may have acquired. The quality of this electricity, *viz.* whether it be poſitive or negative, may be eaſily aſcertained, ſince the electricity of the metal plate muſt be the contrary of that acquired by the body rubbed upon it. Mr. CAVALLO alſo made uſe of this method to diſcover the electricity of certain bodies *. But there is a better method, to be uſed in caſe the bodies to be examined are not eaſily adapted to the metal plate, which method neither Mr. CAVALLO nor others have known. This is the following. The metal plate being laid upon the imperfectly conducting plane, the body to be tried is rubbed againſt, or is repeatedly ſtroked, upon it; which done, the plate is taken up, and is examined by an electrometer. If the body tried by this method is a piece of leather, a ſtring, a piece of cloth, or velvet, or other imperfect conductor of the like ſort, the plate will be certainly found electrified, and incomparably more by this means than if it were ſtroked by the ſame bodies, whilſt ſtanding inſulated in the air. In ſhort, by either of thoſe methods you will obtain ſome electricity from ſuch bodies as could hardly be expected to give any, even when they are not very dry. Indeed, coals and metals excepted,

* See his Treatiſe on Electricity, part IV. chap. VI.

every

every other body will give ſome electricity. I can farther ſay, that I have often obtained ſome electricity even by ſtroking the metal plate with my naked hand.

28. It has been queſtioned, whether evaporation, fermentation, &c. produced any electricity, and the inveſtigation is of conſequence for determining ſomething certain about the atmoſpherical electricity. I know that various perſons have attempted in vain to diſcover electricity in thoſe caſes. Some experiments of mine relating to this purpoſe had alſo failed; nevertheleſs, I entertained ſome hopes of ſucceeding, as I had for a great while imagined, that efferveſcence, diſſolution, evaporation, &c. by diſturbing the natural form and ſituation of the particles of bodies, ought to have increaſed or diminiſhed the capacity of the bodies contiguous to thoſe in action, and conſequently ought to have occaſioned in ſome caſes a rarefaction, and in others a condenſation of the electric fluid. Being perſuaded of this theory, I thought that the electricity produced in thoſe caſes was not diſcovered, partly becauſe of its ſmall quantity, and partly becauſe the inſulation was almoſt deſtroyed by the vapours that roſe, and I imagined, that by a greater accuracy, and by multiplying the experiments, I ſhould ſome time or other diſcover it *. It is about two years ſince, that having gradually been able to condenſe the electricity to a great degree by means of the above deſcribed apparatus, I again thought of repeating my old experiments about the evaporation, &c. and entertained much better hopes of diſcovering ſomething new about it, almoſt foreſeeing the event; but various occupations deferred thoſe experiments till the months of March and April of the preſent year 1782, when being at Paris, in company with ſome members of the Royal Academy of Sciences, I at laſt ſucceeded in obtaining clear ſigns of electricity, nay and even the ſpark, from the evaporation of water, from the ſimple combuſtion of coals,

* All theſe thoughts are mentioned in a Latin diſſertation, printed in the year 1769, and entitled, *De vi attractiva ignis electrici, ac phænomenis inde pendentibus, ad* JOHANNEM BAPTISTAM BECCARIAM, &c.

and

and from various effervescences, as those which produce inflammable air, fixed air, and nitrous air.

29. I shall finish the first part of this paper with observing, that besides the abovementioned uses, to which our condensing apparatus may be applied, the various experiments which may be made with it throw great light upon the theory of electric atmospheres in general, of which we are going to treat in the second part.

PART II.

30. The experiments related in the foregoing pages have shewn how easily a metal plate, or other conducting plain surface, when properly situated, can draw the electric fluid upon itself from a weak atmospherical electricity, from a Leyden phial, &c. so as to render its effects much more conspicuous and vigorous. It is now necessary to give an explanation of those phenomena, the theory of which will greatly facilitate the practical performance of this sort of experiments.

31. The whole matter, therefore, may be reduced to this, *viz.* that the metal plate has a much greater capacity for holding electricity in one case, *viz.* when it lies upon a proper plane (as mentioned in § 11. 12. 22.) than when it stands quite insulated, as when it is suspended in the air by its silk strings or insulating handle, or when it stands upon an insulating stand, as a thick stratum of resin or the like.

32. It is easy to comprehend, that wherever the capacity of holding electricity is greater, there the intensity of electricity

is proportionably leſs, *viz.* a greater quantity of electricity is in that caſe required, in order to raiſe its intenſity to a given degree; ſo that the *capacity* is inverſely as the *intenſity*, by which word I mean the endeavour by which the electricity of an electrified body tends to eſcape from all the parts of it, to which tendency or endeavour the electrical phenomena of attraction, repulſion, and eſpecially the degree of elevation of an electrometer, correſpond.

33. That the *intenſity* of electricity muſt be inverſely proportional to the *capacity* of the body electrified, will be clearly exemplified by the following experiment. Take two metal rods of equal diameter, but one of them a foot, and the other five feet long; and let the firſt be electrified ſo high as that the index of an electrometer annexed to it may be elevated to 60°; then let this electrified rod touch the other rod, and in that caſe it is evident, that the intenſity of the electricity, by being parted between the two rods, will be diminiſhed in proportion as the capacity is increaſed; ſo that the index of the electrometer, which before was elevated to 60°, will now fall to 10°, *viz.* to one-ſixth of the former intenſity, becauſe now the capacity is ſix times greater than when the ſame quantity of electricity was confined to the firſt rod alone. For the ſame reaſon, if the ſaid quantity of electricity was to be communicated to a rod ſixty times longer, its intenſity would be diminiſhed to one degree; and, on the contrary, if the electricity of this long conductor was to be contracted into the ſixtieth part of that capacity, its intenſity would be increaſed to 60°.

34. Now not only conductors of different bulk have different capacities for holding electricity, but alſo the capacity of the ſame conductor may be increaſed or diminiſhed by various circumſtances, ſome of which have not yet been properly conſidered. It has been obſerved, that the capacity of the ſame conductor is increaſed or diminiſhed in proportion as its ſurface is enlarged or contracted, as is ſhewn by Dr. FRANKLIN's experiment of the can and chain, and various other experiments, from which it has been concluded, that the capacity of conductors

ductors is in proportion to their ſurface, and not to their quantity of matter.

35. This concluſion is true, but does not comprehend the whole theory, ſince even the extenſion contributes to increaſe the capacity; ſo that of two conductors, which have equal but diſſimilar ſurfaces, that which is the more extended in length has the greater capacity *. In ſhort, it appears from all the experiments hitherto made, that the capacity of conductors is in proportion not to the ſurfaces in general, but *to the ſurfaces which are free*, or *uninfluenced by an homologous atmoſphere*.

36. But that which comes nearer to our caſe is, that the capacity of a conductor, which has neither its form nor ſurface altered, is increaſed when, inſtead of remaining quite inſulated, the conductor is preſented to another conductor not inſulated; and this increaſe is more conſpicuous, according as the ſurfaces of thoſe conductors are larger and come nearer to each other.

When an inſulated conductor is oppoſed or preſented to another conductor whatever, I call it a *conjugate conductor*.

37. The circumſtance mentioned in the preceding paragraph, which augments prodigiouſly the natural capacity of conductors, is that which I find to have been hitherto principally overlooked, far from any advantages having been deduced from it; but let us begin with thoſe experiments which ſhew this increaſed capacity in the ſimpleſt manner. I take, for example, the metal plate of an electrophorus, and holding it by its inſulating handle in the air, electrify it ſo high that the index of an electrometer annexed to it might be elevated to 60°, then lowering this metal plate by degrees towards a table or other conducting plain ſurface, I obſerve that the index of the electrometer falls gradually from 60° to 50°, 40°, 30°, &c. Notwithſtanding this appearance, the quantity of electricity in the plate remains the ſame, except the ſaid plate be brought ſo near the table as to occaſion a tranſmiſſion of the electricity from the

* See my Diſſertation on the Capacity of Conductors, publiſhed at Milan in the *Opuſcoli Scelti* for the year 1778; and alſo in ROZIER's Journal for the enſuing year.

former to the latter; at leaſt the quantity of electricity will remain as much the ſame as the dampneſs of the air, &c. will permit. The decreaſe, therefore, of intenſity is owing to the increaſed capacity of the plate, which now is not inſulated *ſolitary* but *conjugate*. In proof of this propoſition, if the plate be removed gradually farther and farther from the table, it will be found, that the electrometer riſes again to its former ſtation, namely to 60°, excepting the loſs of that quantity of electricity, which during the experiment muſt have been more or leſs imparted to the air, &c.

38. The reaſon of this phenomenon is eaſily derived from the action of electric atmoſpheres. The atmoſphere of the metal plate, which for the preſent I ſhall ſuppoſe to be electrified poſitively, acts upon the table or other conductor whatever to which it is preſented; ſo that the electric fluid of the table, agreeably to the known laws, retiring to the remoter parts of it, becomes more rare in thoſe parts which are expoſed to the metal plate, and this rarefaction becomes greater the nearer the electrified metal plate is brought to the table. If the metal plate is electrified negatively, then the contrary effects muſt take place. In ſhort, the parts immerſed into the ſphere of action of the electrified metal plate, contract a contrary electricity, which *accidental* electricity, making in ſome manner a compenſation for the *real* electricity of the metal plate, diminiſhes its intenſity, as is ſhewn by the depreſſion of the electrometer (§ 27.).

39. The two following experiments will throw more light upon the reciprocal action of the electric atmoſpheres. Firſt, ſuppoſe two flat conductors, electrified both poſitively or both negatively, to be preſented towards, and to be gradually brought near, each other: it will appear, by two annexed electrometers, that the nearer thoſe two conductors come to each other, the more their intenſities will increaſe; which ſhews, that either of the two *conjugate* conductors has a much leſs capacity now than when it was ſingly inſulated, and out of the influence of the other. This experiment explains the reaſon why an electrified conductor will ſhew a greater intenſity

when

when it comes to be contracted into a ſmaller bulk; and alſo why a long extended conductor will ſhew a leſs intenſity than a more compact one, ſuppoſing that their quantity of ſurface and of electricity is the ſame; becauſe the homologous atmoſpheres of their parts interfere leſs with each other in the former than in the latter caſe.

40. Secondly, Let the preceding experiment be repeated with this variation only, *viz.* that one of the flat conductors be electrified poſitively, and the other negatively; the effects then will be juſt the reverſe of the preceding, *viz.* the intenſity of their electricities will be diminiſhed, becauſe their capacities are increaſed the nearer the conductors come to each other.

41. Let us now apply the explanation of this laſt experiment to the other experiment mentioned in § 38. *viz.* that of bringing the electrified metal plate towards a conducting plane which is not inſulated; for as this plane acquires a contrary electricity, it follows, that the intenſity of the metal plate's electricity muſt be diminiſhed; hence the annexed electrometer is depreſſed according as the capacity of the plate is increaſed, and conſequently the plate in that caſe may receive a greater quantity of electricity.

42. This matter may be rendered ſtill more clear by inſulating the conducting plane, whilſt the other electrified plate is upon it, and afterwards ſeparating them; for then both the metal plate and the conducting plane (which may be called the *inferior* plane) will be found electrified, but poſſeſſed of contrary electricities, as may be aſcertained by electrometers.

43. If the inferior plane is inſulated firſt, and then the electrified plate is brought over it, then the latter will cauſe an endeavour in the former to acquire a contrary electricity, which, however, the inſulation prevents from taking place; hence the intenſity of the electricity of the plate is not diminiſhed, at leaſt the electrometer will ſhew a very little and almoſt imperceptible depreſſion, which ſmall depreſſion is owing to the imperfection of the inſulation of the inferior plane, and to the ſmall rarefaction and condenſation of the electric fluid, which may take place in different parts of the ſaid inferior plane.

plane. But if in this ſituation the inferior plane be touched ſo as to cut off the inſulation for a moment, then it will immediately acquire the contrary electricity, and the intenſity in the metal plate will be diminiſhed.

44. If the inferior plane, inſtead of being inſulated, were itſelf a non-conducting ſubſtance, then the ſame phenomena would happen, *viz.* the intenſity of the electrified metal plate laid upon it would not be diminiſhed. This, however, is not always the caſe; for if the ſaid inferior non-conducting plane is very thin, and is laid upon a conductor, then the intenſity of the electrified metal plate will be diminiſhed, and its capacity will be increaſed by being laid upon the thin inſulating ſtratum; becauſe in that caſe the conducting ſubſtance, which ſtands under the non-conducting ſtratum, acquiring an electricity contrary to that of the metal plate, will diminiſh its intenſity, &c. and then the inſulating ſtratum will only diminiſh the mutual action of the two atmoſpheres more or leſs, according as it keeps them more or leſs aſunder.

45. The intenſity or electric action of the metal plate, which diminiſhes gradually as it is brought nearer and nearer to a conducting plane not inſulated, becomes almoſt nothing when the plate is nearly in contact with the plane, the compenſation or accidental balance being then almoſt perfect. Hence, if the inferior plane only oppoſes a ſmall reſiſtance to the paſſage of the electricity (whether ſuch reſiſtance is occaſioned by a thin electric ſtratum, or by the plane's imperfect conducting nature, as is the caſe with dry wood, marble, &c.): that reſiſtance joined to the interval, however ſmall, that is between the two planes, cannot be overcome by the weak intenſity of the electricity of the metal plate, which on that account will not dart any ſpark to the inferior plane (except its electricity were very powerful, or its edges not well rounded) and will rather retain its electricity; ſo that, being removed from the inferior plane, its electrometer will nearly recover its former height. Beſides, the electrified plate may even come to touch the imperfectly conducting plane, and may remain in that ſituation for ſome time; in which caſe the intenſity being reduced

almoſt to nothing, the electricity will paſs to the inferior plane exceedingly ſlowly.

46. But the caſe will not be the ſame if, in performing this experiment, the electrified metal plate be made to touch the inferior plane *edgewiſe*; for then its intenſity being greater than when laid flat, as appears by the electrometer, the electricity eaſily overcomes the ſmall reſiſtance, and paſſes to the inferior plane, even acroſs a thin ſtratum *; becauſe the electricity of one plate is balanced by that of the other, only in proportion to the quantity of ſurface which they oppoſe to each other within a given diſtance: whereby when the metal plate touches the other plane in flat and ample contact its electricity is not diſſipated. This apparent paradox is clearly explained by the theory of electric atmoſpheres.

47. What looks more like a paradox is, that neither will the touching the metal plate with a finger, or with a piece of metal, deprive it of all its electricity, whilſt ſtanding upon the proper plane; ſo that it generally leaves it ſo far electrified that, when it is afterwards ſeparated from that plane, it will ſtill afford a ſpark. Indeed this phenomenon could not be explained upon the ſuppoſition that the finger or the metals were perfect conductors. But ſince we do not know of any perfect conductor, the metals or the finger, oppoſe a reſiſtance ſufficient to retard the immediate diſſipation of the electricity of the

* This explanation, properly applied, renders evident the actions of *points* in general. Properly ſpeaking, a pointed conductor, not inſulated, when preſented to an electrified body, has not in itſelf any particular virtue of attracting electricity. It acts only like a conductor not inſulated, which does not oppoſe any reſiſtance to the paſſage of the electric fluid. If the ſame conductor, inſtead of being pointed, was to preſent a globular or flat ſurface to the electrified body, neither would it in that caſe oppoſe a greater reſiſtance to the paſſage of the electricity. But the reaſon why the electricity will not paſs nearly ſo eaſily from the electrified body to the conductor when it is flat or globular, as when it is pointed, is becauſe in the former caſe the intenſity of the electricity in the electrified body is weakened by the oppoſed flat ſurface, which, acquiring the contrary electricity, compenſates the diminiſhed intenſity incomparably more than a point can. It appears, therefore, that it is not the particular property of a point or of a flat ſurface, but the different ſtate of the electrified body, that makes it part with its electricity eaſier, and from a greater diſtance, when a pointed conducting ſubſtance, than when a flat or globular one is preſented to it.

plate,

plate, which is in that caſe actuated by a very ſmall degree of intenſity or endeavour of expanding; ſo that ſuppoſe, for inſtance, that the piece of metal, or the finger by touching the plate, took off ſo much of its electricity as to reduce the intenſity of the remainder to the fiftieth part of a degree; this remaining electricity would then be almoſt nothing; but when the plate, by being ſeparated from the inferior plane, has its capacity ſo far diminiſhed as to render the intenſity of its electricity 100 times greater, then the intenſity of that remaining electricity would become of two degrees or more, *viz.* ſufficient to afford a ſpark.

48. Hitherto we have conſidered in what manner the action of electric atmoſpheres muſt modify the electricity of the metal plate in its various ſituations. We muſt now conſider the effects which take place when the electricity is communicated to the metal plate whilſt ſtanding upon the proper plane. The whole buſineſs having been proved in the preceding pages, it is eaſy to deduce the applications from it; nevertheleſs, it will be uſeful to exemplify it by an experiment. Suppoſe that a Leyden phial or a conductor were ſo weakly electrified that the intenſity of its electricity was only of half a degree or even leſs: if the metal plate of our apparatus, when ſtanding upon the proper plane, was to be touched with that phial or conductor, it is evident, that either of them would impart to it a quantity of its electricity, proportional to the plate's capacity, *viz.* ſo much of it as ſhould make the intenſity of the electricity of the plate equal to that of the electricity in the conductor or phial, ſuppoſe of half a degree; but the plate's capacity, now that it lies upon the proper plane, is above 100 times greater than if it ſtood inſulated in the air, or, which is the ſame thing, it requires 100 times more electricity in order to ſhew the ſame intenſity; therefore, in this caſe it muſt require upwards of 100 times more electricity from the phial or conductor. It naturally follows, that when the metal plate is afterwards removed from the proper plane, its capacity being leſſened ſo as to remain equal to the 100th part of what it was before, the intenſity of its electricity muſt become of 50°; ſince, agree-

ably to the ſuppoſition, the intenſity of the electricity in the phial or conductor was of half a degree.

49. A conductor that is electrified whilſt it ſtands in full and ample contact with another proper conductor, as above ſpecified, and is afterwards ſeparated from it, ſhews the ſame phenomena that are exhibited by a conductor, which, after being electrified, is contracted into a ſmaller bulk, or contrariwiſe, like Dr. FRANKLIN's experiment of the can and chain (§ 35.) &c.

50. If a ſmall quantity of electricity applied to the metal plate of the condenſer enables it to give a ſtrong ſpark, it may be aſked, what would a great quantity of electricity do? The anſwer is, that it would do nothing more, becauſe, when the electricity communicated to the metal plate is ſo ſtrong as to overcome the ſmall reſiſtance of the inferior plane, it will be diſſipated.

51. After all that has been ſaid in the preceding pages, it may be eaſily underſtood, that if the metal plate of our condenſer can receive a good ſhare of electricity from a Leyden phial *, or from an ample conductor, however weakly electrified; it cannot receive any conſiderable quantity of it from a conductor of a ſmall capacity; for this conductor cannot give what it has not, except it were continually receiving a ſtream, howſoever ſmall, of electricity, as is the caſe with an atmoſpherical conductor, or with a prime conductor of an electrical machine, which acts very poorly but continues in action. In thoſe caſes it has been obſerved above (§ 4. 25.) that a conſiderable time is required before the metal plate has acquired a ſufficient quantity of electricity.

52. As an ample conductor, weakly electrified, imparts a conſiderable quantity of electricity to the metal plate of our

* In my Paper on the Capacity of ſimple Conductors is ſhewn the great capacity of a Leyden phial in compariſon to its bulk, juſt becauſe the electricity, which is communicated to one of its ſurfaces, is balanced by the contrary electricity of the oppoſite ſurface. There I ſhew, that the capacity of 16 ſquare inches of coated ſurface is equal to the capacity of a conductor made of ſilvered cylindrical ſticks, and nearly 100 feet long, the capacity of which is ſo great that its ſpark occaſions a ſhock conſiderably ſtrong.

condenſer, ſo that when the ſaid metal plate is afterwards ſeparated from its proper plane, the electricity in it appears much condenſed and vigorous; ſo when the ſame metal plate contains a ſmall quantity of electricity, and ſuch as cannot give a ſpark or affect an electrometer, that electricity may be rendered very conſpicuous by communicating it to another ſmall metal plate or condenſer.

Mr. CAVALLO was the firſt who thought of this improvement, which he derived by reaſoning upon my experiments. He actually made a ſmall metal plate not exceeding the ſize of a ſhilling: this ſecond condenſer is certainly of great uſe in many caſes, in which the electricity is ſo ſmall as not to be at all, or not clearly, obſervable by my method or a firſt condenſer only, as has been evidently proved by ſome experiments we made together. Sometimes the uſual metal plate of my condenſer acquired ſo ſmall a quantity of electricity, that being afterwards taken up from the inferior plane, and preſented to an extremely ſenſible electrometer of Mr. CAVALLO's conſtruction, it did not affect it. In this caſe, if the ſaid metal plate, thus weakly electrified, was made to touch the other ſmall plate properly ſituated, and that was afterwards brought near an electrometer, the electricity was then generally ſtronger than what would have been ſufficient to aſcertain its quality.

Now, if by the help of both condenſers the intenſity of the electricity has been augmented 1000 times, which is by no means an exaggeration, how weak muſt then be the electricity of the body examined? how ſmall muſt that electricity be which is produced by rubbing a piece of metal with one's hand, ſince when this electricity is condenſed by both condenſers, and then is communicated to an electrometer, it can hardly affect that inſtrument? Yet it is ſufficient to afford conviction, that the metal can be electrified by the friction of a perſon's hand. Some years ago, *viz.* before the diſcovery of our condenſer, and of Mr. CAVALLO's ſenſible electrometer, we were very far from being able to diſcover ſuch weak excitations; whereas, at preſent, we can obſerve a quantity of electricity incomparably ſmaller than the ſmalleſt obſervable at thoſe times.

APPENDIX.

IN § 28. I mentioned, that after various attempts I at laſt ſucceeded in obtaining undoubted ſigns of electricity from the ſimple evaporation of water, and from various chemical effervescences; but as this is a fact not leſs intereſting than new, it ſeems proper to ſubjoin in this place a faithful account of the experiments made for that purpoſe. The firſt ſet of experiments were made at Paris, in company with Mr. LAVOISIER and Mr. DE LA PLACE, two intelligent philoſophers and members of the Royal Academy of Sciences. After I had ſhewn them my experiments with my condenſer, they, as well as myſelf, began to entertain hopes of ſucceeding in the experiments on the evaporation, &c. Accordingly Mr. LAVOISIER ordered a large condenſer with a marble plane to be made. The firſt experiment I attempted with this inſtrument, in company with Mr. DE LA PLACE, proved unſucceſsful; but the weather at that time was bad, the room was narrow and full of vapours, and the apparatus was not quite in proper order. Mr. DE LA PLACE and Mr. LAVOISIER repeated thoſe experiments in the country, and then they were attended with ſucceſs, which incited us to repeat and diverſify the experiments, by which means the diſcovery was compleated; having obtained unequivocal ſigns of electricity from the evaporation of water, from the ſimple combuſtion of coals, and from the effervescence of iron filings in diluted vitriolic acid. This obſervation was made the 13th of April of the preſent year 1782, and the experiments were performed in the following manner. In an open garden a long metal plate was inſulated, which, by means of a large iron wire, was made to communicate with the metal plate of the condenſer laid upon the piece of

marble, which was kept continually warm by ſome lighted coals ſet underneath. This done, ſome chafing-diſhes, containing burning charcoal, were placed upon the large inſulated plate. The combuſtion of the coals was helped by a gentle wind. Some minutes after, the iron wire, by which the large inſulated plate was connected with the metal plate of the condenſer, was taken off; then the metal plate being removed from the marble by its inſulated handle, and preſented to Mr. CAVALLO's electrometer, made the balls of it diverge with negative electricity. The experiment was repeated by placing upon the large inſulated plate four veſſels, containing iron filings and water, inſtead of the chafing-diſhes: then ſome vitriolic acid was poured into thoſe four veſſels, ſufficient to cauſe a vigorous effervescence, and when the ſtrongeſt ebullition was going to ſubſide, the metal plate of the condenſer was removed from over the marble; and being examined, not only electrified the electrometer with negative electricity, but gave a ſenſible ſpark. At this time having tried to obtain electricity from the evaporation of water, the effects were equivocal or hardly ſenſible; the ſame thing happened a few days after, when however we obtained clear ſigns of electricity from thoſe effervescences, which produce fixed and nitrous air. Thoſe experiments were made in a large room.

One day the electricity ariſing from the evaporation of water ſeemed to be poſitive; but ſubſequent experiments, and other circumſtances, indicate that ſuch a phenomenon muſt be attributed to a miſtake.

Once on repeating theſe experiments in company with Mr. LE ROY, member of the R. A. of Sciences, we could not obtain any electricity from the evaporation of water or from combuſtion, the weather being extremely damp; but the effervescence of iron filings and diluted vitriolic acid produced electricity enough to aſcertain that it was negative, though it afforded no ſpark.

A ſhort time before I left Paris I once more repeated the experiment of the effervescence of iron filings, &c. with ſucceſs. This experiment was made in the laboratory of Mr. BILLAUM, an inſtrument-maker and lover of electricity.

The experiment on the evaporation of water, which did not anſwer ſo well at Paris, ſucceeded much better in London, where I bethought me of throwing water upon the lighted coals, which were kept in an inſulated chafing-diſh. In this manner the electricity of the evaporation never fails to electrify the chafing-diſh negatively, and ſtrongly enough for the electricity to be diſcovered by the ſimple electrometer; it will even afford a ſpark, if the condenſer is uſed. The firſt experiment of this ſort was made at Mr. BENNET's, who is a great lover of electricity, in preſence of Mr. BENNET, Mr. CAVALLO, and Mr. KIRWAN, members of the Royal Society, and of Mr. WALKER, lecturer of experimental philoſophy.

Another time this experiment was repeated with ſucceſs at Mr. CAVALLO's, in the following manner. A ſmall crucible, containing three or four ſmall coals lighted, was inſulated; then a ſpoonful of water was thrown upon the coals, and immediately after an electrometer, which communicated with the coals by means of a wire, diverged with negative electricity.

Theſe are the experiments which I have had the opportunity to make hitherto; in relating which I muſt not omit to obſerve, that although the condenſing apparatus has not been always indiſpenſably neceſſary, Mr. CAVALLO's very ſenſible electrometer alone having been often ſufficient for the purpoſe; yet it muſt be confeſſed, that it was the condenſing apparatus which ſuggeſted theſe experiments, and by the help of which even the electric ſpark could be obtained. Theſe experiments have juſt opened the way to a vaſt field, which deſerves much farther inveſtigation. It is natural to ſuppoſe, that if compact bodies, when they are rarefied or become an elaſtic fluid, require an additional quantity of electric fluid, and conſequently leave thoſe bodies, with which they are connected, negatively electrified; it muſt happen, on the contrary, that when vapours condenſe they muſt part with ſome electric fluid, that is, muſt produce a poſitive electricity. This, however, remains to be proved experimentally, and I have already imagined ſeveral ways of trying it, which will be put in practice as ſoon as I ſhall have the opportunity. Mean while I beg leave to conclude

clude this paper with mentioning a few ideas I entertain relating to the atmoſpherical electricity.

The experiments hitherto made, though not numerous, yet concur to ſhew, that the vapours of water, and in general the parts of all bodies, that are ſeparated by volatilization, carry away an additional quantity of electric fluid as well as of elementary heat, and conſequently that thoſe bodies, from the contact of which the volatile particles have been ſeparated, remain both cooled and electrified negatively: from which it may be deduced, that whenever bodies are reſolved into volatile elaſtic fluid, their capacity for holding electric fluid is augmented, as well as their capacity for holding common fire, or the calorific fluid. This is a ſtriking analogy by which the ſcience of electricity throws ſome light upon the theory of heat, and alternately derives light from it; I mean on the doctrine of latent or ſpecific heat, the firſt notions of which were ſuggeſted by the admirable experiments of Dr. BLACK and WILKE, and which has been afterwards much elucidated by Dr. CRAWFORD, who followed the experiments of Dr. IRWIN.

By following this analogy it ſeems, that as the vapours on their condenſing, loſe part of their latent heat, on account of their capacity being diminiſhed, ſo they part with ſome electric fluid. Hence originates the poſitive electricity, which is always more or leſs predominant in the atmoſphere, when the ſky is clear, *viz.* at that height where the vapours begin to be condenſed. Accordingly, the atmoſpherical electricity is ſtronger in fogs, in which caſe the vapours are more condenſed, ſo as to be almoſt reduced into drops, and is ſtill ſtronger when thick fogs become clouds.

Hitherto we have accounted for the poſitive atmoſpherical electricity; but it is eaſy to account for clouds negatively electrified; for when a cloud, poſitively electrified, has been once formed, its ſphere of action is extended a great way round, ſo that if another cloud comes within that ſphere, its electric fluid, agreeably to the well known laws of electric atmoſpheres, muſt retire to the parts of it which are the remoteſt from the firſt cloud; and from thence the electric fluid may be communicated

nicated to other clouds, or vapours, or terreſtrial prominencies. Thus a cloud may be electrified negatively, which cloud, after the ſame manner, may occaſion a poſitive electricity in another cloud, &c. This explains not only the negative electricity, which is often obtained from the atmoſphere in cloudy weather; and the frequent changes from poſitive to negative electricity, and contrariwiſe in ſtormy weather; but alſo the waving motion often obſerved in the clouds, and the hanging down of them, ſo as nearly to touch the earth.

After the fore-mentioned diſcoveries we need no longer wonder at the appearance of lightnings in the eruptions of volcanos, as was particularly obſerved in the late dreadful eruption of Mount Veſuvius. The few experiments I have made ſhew, that the quantity of ſmoke, but much more the rapidity with which it is produced, tends to increaſe the electricity which ariſes from combuſtion, &c. How great muſt then be the quantity of electricity that is produced in ſuch eruptions?

AMENDMENT to P. 191. L. 12.

I have lately repeated this experiment, and found that one measure of alkaline air is saturated by less than half of one measure of fixed air, but more than one-third, conformably to Dr. PRIESTLEY's first experiment, p. 293.; by which it appears, that 100 gr. of alkaline air require about 120 of fixed air to saturate them: and hence 100 gr. of concrete volatile alkali contain about 53 of fixed air, 44 of mere volatile alkali, and 3 of water.

ERRATA.

VOL. LXXI.

Page.	Line.	
9.	20.	*for* 0,03 *read* 0,035
11.	26.	*for* 3,100 *read* 3106.
18.	12.	*for* $\frac{3,55}{0,290}$ *read* $\frac{3,55}{0,299}$

VOL. LXXII.

73.	12.	*for* one fortieth *read* one twentieth
75.	1.	*for* five ſevenths *read* five fourths
78.	9.	*for* ſevenths of an inch *read* fourths of an inch
81.	2.	*for* now *read* more
117.	11.	*for* 38 *read* 39
127.	8.	*for* 108 *read* 107
136.	24.	*for* ζ Capricorni *read* FL. 64. Sagittarii
147.	3.	*for* the $1\frac{1}{2}$ *read* the ſartheſt $1\frac{1}{2}$
149.	28.	*for* 40′ *read* 40″
153.	7.	*for* ϰ FL. 10. *read* ζ FL. 11.
155.	4.	*for* 5 *read* 4
161.	21.	*for* 6″ 27′ *read* 6″ 27‴
182.		*at the top of the* 6*th column, inſert* Phyſical ſpecific gravity.
——		*firſt ſeries of the* 6*th column, for* 1,846 *read* 1,8846
183.		*at the top of the* 6*th column, inſert* Phyſical ſpecific gravity.
185.	12.	*for* 1,4653 *read* 1,4650
206.	14.	*after* nitrous *read* teſt
217.	23. and 26.	*for* 1 PR. *read* 4 PR.